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The Journal of HEREDITY

Early Experiments on Sex and Hybridization in Plants

"Flightless" Fowls

Varietal Differences in Nematode Resistance

"Iarovization"—A Technique of Interest to Geneticists and Breeders

Are There Identical Twins in Sheep?

VOLUME 23 NOVEMBER, 1932 NUMBER 11

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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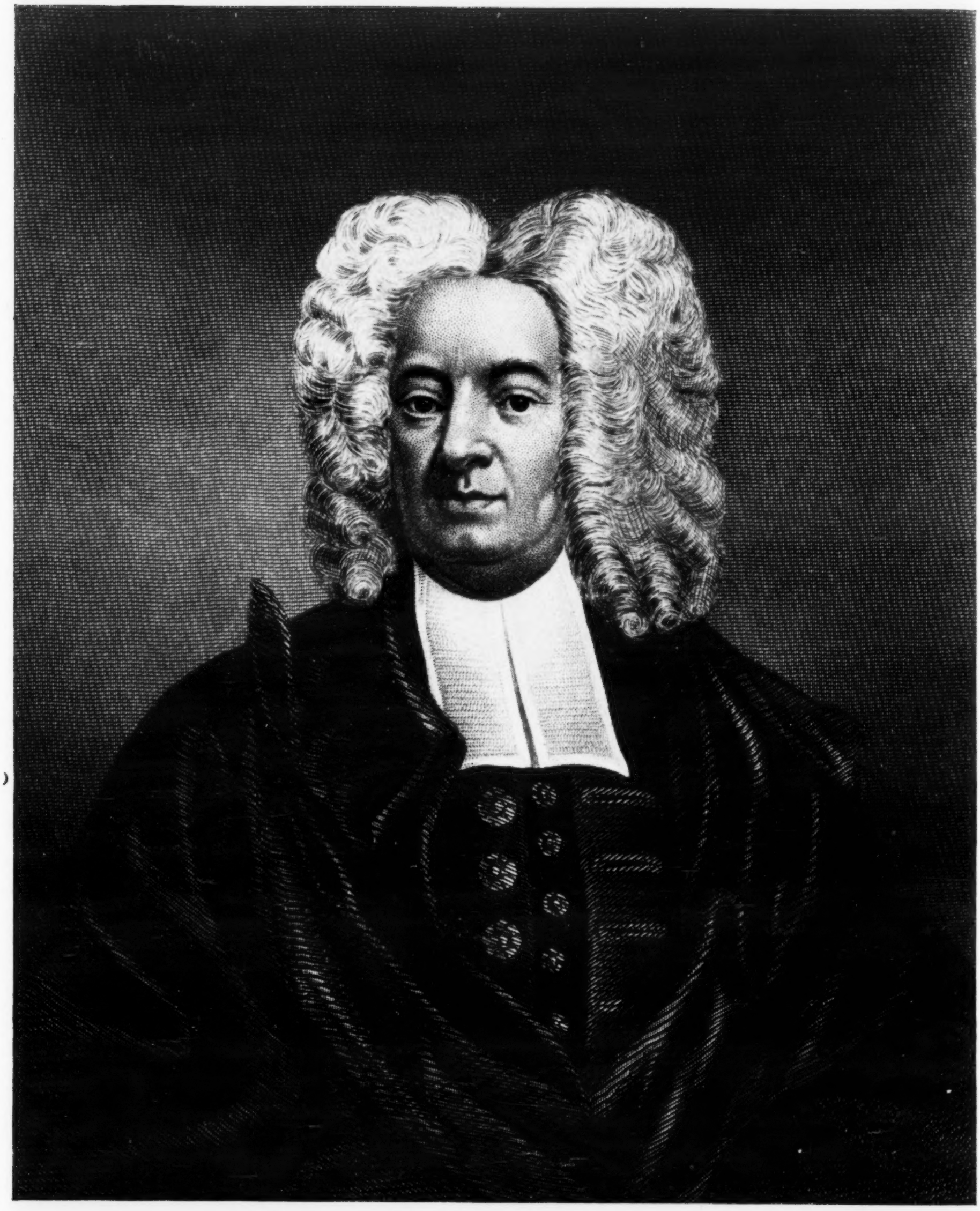

Cotton Mather

FIRST OBSERVED HYBRIDIZATION IN PLANTS

His witch-hunting activities have led posterity to overlook the fact that Cotton Mather "contributed my two mites to the way wherein vegetation is carried on," which contribution is the earliest recorded observation of natural hybridization in plants!

SOME FORGOTTEN RECORDS OF HYBRIDIZATION AND SEX IN PLANTS

1716-1739

CONWAY ZIRKLE
Department of Botany, University of Pennsylvania

THE first half of the eighteenth century was a time of exceptional botanical activity in the English colonies along the Atlantic Coast of North America. A new country filled with strange and interesting plants had just been opened for exploration and European botanists were anxious for specimens. Medicinal plants were in great demand and any educated colonists who sent seed or herbaria sheets to England or the Continent could be assured of an interesting and profitable correspondence with the plant importers.

The colonists who collected, systematized and recorded the distribution of the new plants seem to have been exceptionally able. They kept in close touch with the development of botany in Europe, visited and wrote letters to each other and exchanged ideas and specimens. They imported microscopes, investigated plant anatomy, especially the anatomy of the flower, and devised a number of physiological experiments. The most prominent men in the colonies were keenly interested in this local scientific development and some even joined in the experimentation.

The European botanists were certainly informed of the work of the Americans, although the very real contributions made by the latter, in fields other than taxonomy, have been, with a single exception, completely forgotten.

Most biologists have been so impressed by the overwhelming adequacy of Sachs' *History of Botany* (1530-1860) that any important botanical contribution which he failed to record is apt to remain unknown. Moreover, so greatly is Sachs' judgment admired and his fairness recognized (except perhaps in his treatment of Linnaeus), that he has become the principal arbiter of priority claims even in such a confused subject as that of the discovery of sex in plants. Sachs' evaluation of the contributions of the different workers in this field is still generally accepted.

In order to prove that plants reproduce sexually it is necessary to show, first, that viable seeds can not be produced without the cooperation of some element (pollen) which might be interpreted as male, and, second, that both this hypothetical male element and the egg bear factors which influence the progeny. Camerarius (1694) is cited by Sachs as the first investigator to prove experimentally that pollen is necessary for seed development, while Koelreuter (1761) is credited with having made the first systematic study of plant hybridization, proving incidentally that both parents contribute to the offspring. Sachs rightly emphasized the importance of Koelreuter's work for, as far as he knew, there were no reliable records of plant hybrids before 1761. In fact he mentions only two instances of species crossing before Koelreuter's experiments and both were considered somewhat dubious. He said of the first, "Bradley is our authority for the statement that a gardener in London had obtained a hybrid between *Dianthus caryophyllus* and *Dianthus barbatus* by artificial means as early as 1719" (Sachs, p. 406). The second instance is recorded just as briefly, "Soon after allusion is made (by Linnaeus, 1735) to the artifices used by gardeners to obtain hybrid tulips and cabbages, but the matter is treated rather as agreeable trifling" (Sachs, p. 400). Haartman on

purely taxonomic grounds had interpreted some of his finds as natural hybrids.

Cotton Mather

Forty-five years before Koelreuter crossed different species of *Nicotiana,* however, an American, apparently unknown to Sachs, had observed three important phenomena: (1) wind pollination; (2) hybridization (variety and perhaps species crosses); and (3) the resemblance of the offspring to the male parent.

Cotton Mather is better known today as a witch-hunter than as a member of the Royal Society. While he was undoubtedly very credulous and believed on very scanty evidence much that was later shown to be false, he at least kept in touch with the scientific progress of his time and contributed what he could to the advancement of knowledge. We know that he was informed of the new hypothesis, that the flowering plants reproduced sexually, and he evidently accepted the views advanced by Nehemiah Grew, as is shown by the following passage (Mather, 1721):

> The *Stamina,* with their *Apices;* and the *Stylus* (called the *Attire* by Dr. *Grew*) which is found a sort of *Male Sperm,* to impregnate and fructify the Seed!

The observations of Cotton Mather, which should take their proper place in the history of botany, were described by him in a letter to James Petiver, F. R. S., dated September 24, 1716. This letter, which is now in the files of the British Museum (Sloane Ms. 4065, fol. 255), has probably never been published, but Kittredge (1916) has summarized its contents. In it Mather described hybridization in *Zea* and *Cucurbita.* He repeated the description in his book, *Religio Philosophica;* or *The Christian Philosopher,* London, 1721. The following passage is taken from Essay XXVI, *Of the* VEGETABLES.

> That I may a little contribute my *two Mites* to the illustration of the way wherein *Vegetation* is carried on, I will here communicate a couple of Experiments lately made in my Neighbourhood.
>
> My Neighbour planted a Row of Hills in his Field with our *Indian Corn,* but such a Grain as was colour'd *red* and *blue;* the rest of the Field he planted with Corn of the most usual Colour, which is *yellow.* To the most *Windward-side* this Row infected *four* of the next neighbouring Rows, and part of the fifth, and some of the sixth, to render them colour'd like what grew on itself. But on the *Leeward-side* no less than seven or eight Rows were so colour'd, and some smaller impressions were made on those that were yet further distant.
>
> The same Neighbour having his Garden often robb'd of the *Squashes* growing in it, planted some *Gourds* among them, which are to appearance very like them, and which he distinguish'd by certain adjacent marks, that he might not himself be imposed upon; by this means the Thieves 'tis true found a very *bitter Sauce,* but then all the *Squashes* were so infected and embitter'd, that he was not himself able to eat what the Thieves had left of them.

As the bitterness of the squashes following their mesalliance with the gourds appears a generation too soon, it is regretable that a more careful record was not kept. For until we can trace Fairchild's hybridization of *Dianthus* to some period before 1717, Mather's account is the earliest we have of possible species crossing in plants.*

The spontaneous crossing of different varieties of *Zea Mays* was certain to attract attention sooner or later. Doubtless many more records will be found when the documents of this period have been examined adequately. It is not strange that Cotton Mather's account of cross pollination was overlooked as the biologists of his time were suspicius of the New England "Saints" and did not search their theological works for scientific data. However, it is remarkable that other records of this hybridization have not received more attention.

---

*The gourds were *Cucurbita Pepo* var. *ovifera,* the squashes were probably either *C. Pepo* var. *condensa* or *C. maxima;* if the latter, Mather reported a real species cross.

Thomas Fairchild

The first artificial hybrid, of which we have a record, was made by Mr. Thomas Fairchild, of Hoxton, when he crossed *Dianthus caryophyllus* with *Dianthus barbatus*. Although Fairchild's contemporaries gave him full credit for his achievement, Sachs minimized it and the fashion was set which has lasted unchallenged to the present. Sachs[31] did not even mention Fairchild's name, but merely referred to him as a "gardener in London." Focke[15] cited Fairchild's successful experiment (pp. 55, 430) but added, "This success in artificial fertilization was neither used for the advancement of science nor does it seem to have given gardeners any stimulus for further research." Pfeffer[28] went even further by stating (p. 265), "Die wohl frűher (1719) ausgeffűhrt Kreuzung zweier Nelken durch *Fairchild* war ein rein gärtnerischer Versuch, der keine wissenschaftigen Bedeutung erlangte." Strangely enough even the recent English and American historians of botany (Green,[16] Roberts[30]) have failed to combat this completely unfair estimate of Fairchild's contribution.

Unfortunately no one has found a really satisfactory description of the *Dianthus* hybridization. Botanists, since Sachs, have seemingly known of this work through a single reference in Bradley's *New Improvements in Gardening, etc.*, which is uniformly dated 1719. This reference is quoted in part by both Green[16] and Roberts.[30] While this record is better than any other yet found, it is incomplete and should be supplemented by other citations. The following quotation is taken from the first edition of Bradley's work which appeared in 1717. Fairchild's experiments were, of course, performed earlier. From Bradley (1717), part I, pp. 23, 24:

> I believe I need not explain how the *Male Dust* of *Plants* may be convey'd by the Air from one to another, by which the *Generation* and *Production* of new Plants is brought about . . . . Moreover a curious Person may by this knowledge produce such rare Kinds of *Plants* as have not yet been heard of, by making choice of two *Plants* for his Purpose, as near alike in their Parts, but chiefly in their *Flowers* or *Seed-Vessels*; for example the *Carnation* and *Sweet William* are in some respects alike; the Farina of one will impregnate the other, and the *Seed* so enliven'd will produce a *Plant* differing from either, as may now be seen in the Garden of *Mr. Thomas Fairchild*, of Hoxton, a Plant neither *Sweet William* nor *Carnation*, but resembling both equally; which was raised from the *Seed* of a *Carnation* that had been impregnated by the *Farina* of the *Sweet William*. These Couplings are not unlike that of the *Mare* with the *Ass* which produces the *Mule*, and in regard to *Generation*, are also the same with *Mules*, not being able to multiply their *Species*, no more than other Monsters generated in the same manner.

Bradley suggests a practical application of Fairchild's discovery (Part II, pp. 84, 85):

> . . . I have endeavour'd to explain how the *Dust* of one Flower will impregnate and enliven the *Seeds* of another, and that from that accidental Coupling the Seeds are so chang'd as to produce Plants with Blossoms varying from those of the Mother plant. I have likewise shewn why double Flowers seldom bear *Seed*, which I conjecture is because the *Male Parts* in them are either not perfect, or else are confin'd from Action by the Multiplicity of the *Petals*. This Consideration leads me to advise the Curious Florists to plant of every good sort of his double Carnations in Beds on a Line in the Middle, and on each Side of them to set at least Two Rows of single ones of choice Colours, and among them some Plants of *Sweet William*, and of the *China* or *Indian Pinks*, which have such Varieties of odd Colours in them, as I shall mention hereafter. The *China Pinks* and the *Sweet Williams*, bearing single Flowers, as well as the single Carnations, may have Opportunities of communicating their *Farina* into the Cells of the double ones, and set their Seeds, which if they do, we shall not only gather a larger Quantity than we could otherwise expect, but likewise be assur'd of great Varieties from them.

The following references supplement Bradley. Miller (1731) under the heading CARYOPHYLLUS, has:

7. CARYOPHYLLUS; *barbatus, hortensis, angustifolius, flore pleno roseo.* The Double Rose-colour'd Sweet John *or* Fairchild's Mule . . . . these continue flowering for a long time, and are extremely beautiful, especially the *Mule*, which produces two full Blooms of Flowers, one in *May*, and the other in *July* . . . .

This passage remained in *The Gardener's Dictionary* until 1754. Perhaps the best description of the hybrid is to be found in a letter of Peter Collinson to John Bartram dated July 22, 1740 (Darlington, 1849).

An instance we have in our gardens, raised by the late Thomas Fairchild, who had a plant from seed, that was compounded of the Carnation and Sweet William. It has the leaves of the first, and its flowers double like the Carnation—the size of a Pink—but in clusters like the Sweet William. It is named a *Mule*—per analogy to the mule produced from the Horse and Ass.

If the following record of the Mule lacks exactitude its source justifies its inclusion. Erasmus Darwin was not only a poet, a physician and a grandfather, but also a well informed botanist. He knew of Fairchild's hybrid and described it in 1781 thus:

Botanic Garden, Part II, Canto IIII
CARYO's sweet smile DIANTHUS proud admires,
And gazing burns with unallow'd desires;
With sighs and sorrows her compassion moves,
And wins the damsel to illicit loves.
The Monster-offspring heirs the father's pride,
Mask'd in the damask beauties of the bride.

When Miller adopted the Linnaean system in 1759 he included the following under the heading "Dianthus":

3. The Mule, or Fairchild's Sweet William, it hath narrower Leaves than either of the former, and is of that Variety called Sweet John: This was said to have been produced from Seeds of a Carnation, which had been impregnated by the Farina of the Sweet William: The Flowers of this are of a brighter red Colour than either of the former, their Bunches are not quite so large, but the Flowers have an agreeable Odour.

When Thomas Martyn revised the dictionary in 1807, the above passage was slightly condensed.

The references thus far quoted do not give us a sufficient basis for judging the character of Fairchild's work. Fairchild himself published very little: *The City Gardener* in 1722 and a short account of some remarkable experiments in the *Philosophical Transactions* of 1724. By far the greater part of his investigations were recorded by his contemporaries: Blair (1720), Bradley (1717, 1721, 1726), Miller (1731) and Collinson (1740). Space forbids an account of Fairchild's many activities. That he was the outstanding commercial florist of his time is shown by many easily obtainable references (Blair, 1720; Bradley, 1717, 1726; Pulteney, 1790; Nichols, 1817; Felton, 1830; Britten and Boulger, 1893). Bradley (1726) referred to Fairchild over twenty times and devoted fifteen pages (Vol. II, pp. 458-473) merely to listing the flowers in the garden at Hoxton with the times of blooming, including in the list the Mule between the Sweet William and Carnation.

It was, however, as an experimental biologist that Fairchild made his chief contributions. At a time when the prestige of the Ancients was still high, when analogy was supposed to furnish proof, and when philosophers still knew enough of the Creator's purposes to arrive at the truth by contemplating the Nature of Things, Fairchild experimented—both for himself and for the more dignified philosophers. His relation to his co-workers is well shown by the honest acknowledgment of Blair in the preface to *Botanical Essays* (1720: . . ."Mr. Fairchild's (*whom I have often mentioned, and to whom I owe all the practical observations I have advanc'd concerning the Vegetation*)." Fairchild also performed many of Bradley's experiments in addition to the most curi-

ous and ingenious ones of his own devising (*Phil. Trans.*, Number 384).

To label the hybridization of Dianthus by the leading experimenter of his generation as "ein rein gärtnerischer Versuch, der keine wissenschaftigen Bedeutung erlangte," in order to give Koelreuter a priority he neither needs nor deserves, would be mildly humorous if such mistatements were not repeated indefinitely.

Philip Miller

Philip Miller was the most widely known botanist of his generation. In his own lifetime his *Gardener's Dictionary* passed through eight full and six abridged editions, together with three editions in foreign languages. Yet in spite of this, his contributions toward establishing the theory of sexual reproduction in plants have been noted only in part and his records of plant hybridization have been ignored.

Miller is credited with being the first investigator to describe insect pollination. Sachs[31,32] erroneously gives the date of this occurrence as 1751, although it had been recorded many times much earlier. From Sachs[31] "Von besonderem Interesse sind die späteren Versuche Müller's (Miller's, in the English translation) von 1751, welche Koelreuter aus dem Gärtnerslexicon (II Theil p. 543) mittheilt, insofern hier zum ersten Mal die Insectenhülfe bei der Bestäubung beobachtet wurde." Sachs, as usual, used Koelreuter's citations and Koelreuter found the reference in the German edition of the *Gardener's Dictionary*, Nurnberg, 1750-51. Green records Miller's observation by quoting directly from the so-called first edition published in 1731, while Roberts quotes from the seventh edition (1759).

Miller is also credited with noting that when female spinach plants were raised apart from the male, they produced seed which contained no embryos. However, his other investigations of sexual reproduction in plants (Cucumbers and Melons) have been left out of the more recent records and his description of spontaneous variety crosses in Cabbage is nowhere evident. This is all the more remarkable because he described his work on *Cucumis* and *Brassica* in many editions of his dictionary.

Miller actually performed his experiments and reported his observations much earlier than our current records would indicate. The date assigned by Sachs, 1751, is of course much too late. Green does not state that Miller's investigations were first published in 1731 but merely quotes from the first folio edition of the dictionary. Miall[24] quotes from the first octavo edition issued in 1724. Miller's first description of his work, however, did not appear in one of his own books. He sent a full account of his observations on insect pollination and his experiments with spinach to Richard Bradley in a letter dated October 6, 1721. Bradley later published this letter in his *Treatise of Husbandry and Gardening,* etc., London, 1726, although an account of Miller's discoveries had already appeared in print.

Miller also informed Mr. Patrick Blair of his discoveries in two letters written on October 19th and November 11th, 1721. Blair recognized the importance of Miller's results and sent an account of them by letter to Sir Hans Sloane. This letter was published in the *Philosophical Transactions,* Number 369.

Observations upon the Generation of Plants, in a letter to Sir Hans Sloane, Bart. Pr. Coll. Med. Patrick Blair, *M.D., F.R.S.*

Boston, Dec. 31, 1721.

Honoured Sir,

It is no small Satisfaction, that what I advanced in my *Botanick Esseys* is now so fully confirm'd by Experiments made by some curious Gardeners, among whom is Mr. *Philip Miller*, who writes me,

November 11, 1721.

1. That in Pursuance of my Advice he separated the Male Plants of the Spinage from the Female; the Consequence was, that the Seeds did swell to the usual Bigness; but when he sow'd it, it did not grow afterwards. He searched into the Seed,

PAUL DUDLEY—JURIST AND NATURALIST

The naturalists of Colonial America were many of them leaders in other fields. Paul Dudley, Attorney General of Massachusetts in 1702, and a fellow of the Royal Society, was one of the first to discuss the effects of cross pollination in maize.

and found it wanted the *Punctum Vitae*, which perhaps might have been the Case with Mr. *Geoffrey*; but if not, the female *Embryones* might have been impregnated another Way, as he experimented with twelve Tulips, which he set by themselves about six or seven Yards from any other, and as soon as they blew, he took out the *Stamina* so very carefully, that he scattered none of the Dust, and about two Days afterwards, he saw Bees working on the Tulips, in a Bed where he did not take out the *Stamina*, and when they came out, they were loaded with the Dust on their Bodies and Legs: He saw them fly into the Tulips, where he had taken out the *Stamina*, and when they came out, he went and found they had left behind them sufficient to impregnate their Flowers, for they were good ripe Seed; which persuades him, that the *Farina* may be carried from Place to Place by Insects, and when they happen upon a Flower, whose *Uterus* is capable to be impregnated by such a Dust, it may be thus effected.

I am of Opinion, this will not suit with Mr. *Morland's* Scheme. For tho' we may suppose the *Stamina* of every Flower to be loaded with a due Proportion of the *Farina*, yet this accidental Conveyance of it to a neighbouring Flower, may be rather less than greater than is necessary: So that, if wanting, then those *Embryones*, which had not received its determined Particle into their Bosom, must be defective in Bulk, or barren in growing, but here all were equally fill'd.

2. By a Second Letter, *October* 19, 1721, he informs me, that he bought a Parcel of *Savoy* Seeds of a Neighbour, which he sowed, and planted out the Plants; but was surprised to see the Production: For he had half of them red Cabbages, and some white Cabbages, and some Savoys with red Ribs, and some neither one Sort nor other, but a Mixture of all Sorts together in one Plant. He went to the Gardiner and told him his Tale, who shew'd him, that he was in the same Condition, but did not know how it should come to pass, for he was sure he took special Care in saving of the Seed. Being ask'd how and where he planted them for Seed, he shew'd him them under a *South-West* Hedge, and told him the Manner in which he planted them: First, a Dozen of white Cabbages, then a Dozen of Savoys, and then a Dozen of Red. Then he immediately thought how it came to pass, by the *Effluvia* impregnating the *Uterus* of one another; and it is very common for our Gardiners to plant white and red Cabbages together for Seed, and they are as often disappointed by having a Degeneracy of both Kinds, which they attribute to the Soil, and think that is the Cause: They send to *Holland* for a fresh Supply of Seeds, and say our Soil will not continue that Sort Good. He told them his Opinion, and they laugh at him for it, and will not be turn'd out of their Road, although they should have never so many Experiments shew'd them . . . . . But it is Time to proceed to another Experiment of my Correspondent, Mr. *Miller*.

Being persuaded to it by an ingenious Gardiner, he pull'd off all the Male-Flowers of some Melon Plants so soon as they appeared; but instead of finding, as his Friend informed him, that these Flowers exhausted the Nourishment from the Fruit; he found that, without these Flowers none of the Melons would grow, for that he was deprived of the Fruit which he expected.

Paul Dudley

One early record of hybridization in *Zea Mays* did receive publicity and was incorporated twice by Philip Miller in the *Gardener's Dictionary*, London, 1731, under the headings "GENERATION" and "MAYS: Indian Wheat." The source of Miller's information concerning the spontaneous crossing of different varieties of this New World form was a paper by Paul Dudley, written in New England and sent to the Royal Society. The communication was entitled "Observations on some of the Plants in New England, with remarkable Instances of the Nature and Power of Vegetation," and was published in the *Philosophical Transactions*, number 385.

Paul Dudley was a member of a distinguished Massachusetts family. In 1702 he became Attorney General of the Colony and in 1718 a Judge of the Superior Court. He was a Naturalist

and a Fellow of the Royal Society. The following extract describing variety crosses in *Zea Mays* is taken from his paper of 1724. It has escaped the attention of the historians of botany.

The mention of *Indian* Corn obliges me to take notice of an extra-ordinary *Phaenomenon* in the Vegetation of that Grain, *viz.* the interchanging, or mixing, of Colours after the Corn is planted. For your better understanding this Matter, I must observe, that our *Indian* Corn is of several Colours, as blue, white, red, and yellow; and if they are planted separately, or by themselves, so that no other Sort be near them, they will keep to their own Colour, *i. e.*, the blue will produce blue, the white, white, *etc.* But if in the same Field, you plant the blue Corn in one Row of Hills (as we term them) and the white, or yellow, in the next Row, they will mix and interchange their Colours; that is, some of the ears of Corn, in the blue Corn Rows, shall be white, or yellow; and some again, in the white or yellow Rows, shall be of a blue Colour. Our Hills of *Indian* Corn are generally about four Foot assunder, and so continued in a straight Line, or Row of Hills, and so on; and yet this mixing and interchanging of Colours has been observed, when the D'stance between the Rows of Hills, has been several Yards; and a worthy Clergyman, of an Island in this Province (*The Reverend Mr.* Mayhew, *of* Martha's *Vineyard*), assures me that the blue Corn has thus communicated, or exchanged, even at the Distance of four or five Rods; and, particularly in one Place, where there was a broad Ditch of Water betwixt them. Some of our People, but especially the *Ab-Origines*, have been of the Opinion, that this Commixtion, and Interchange, was owing to the Roots, and small Fibres reaching to and communicating with one another; but this must certainly be a Mistake, considering the great Distance of the Communication, especially at some Times, and cross a Canal of Water; for the smallest Fibres of the Roots of our *Indian* Corn, cannot extend above four or five Foot. I am therefore humbly of Opinion, that the *Stamina*, or Principles of this wonderful Copulation, or mixing of Colours, are carried thro' the Air by the Wind; and that the Time, or Season of it, is, when the Corn is in the Earing, and while the Milk is in the Grain, for at that Time, the Corn is in a Sort of Estuation, and emits a strong Scent. One Thing, which confirms the Air's being the Medium of this Communication of Colours in the Corn, is an Observation of one of my Neighbours, that a close, high board Fence, between two Fields of Corn that were of a different Colour, entirely prevented any Mixture or Alteration of Colour, from that they were planted with.

The above communication was the basis of all of Miller's accounts of the variety crosses in *Zea Mays* which appeared in the successive editions of *The Gardener's Dictionary*. I have seen it in the 1st Octavo Edition (1724), 1st Folio edition (1731), 2nd edition (1733), 7th edition (1759) and 9th edition (1807), and also in the 4th and 5th Octavo (Abridged) Editions (1754, 1763).

James Logan

Another American to recognize the advantages offered by *Zea Mays* for an investigation of sex in plants was James Logan, a Colonial Governor of Pennsylvania. He designed several scientific experiments, duly ran the necessary controls, and succeeded in showing that Indian corn would not produce grain if pollen were kept away from the stigmas. An account of this work was written in Latin and published in Holland (*Experimenta et Maletemata de Plantarum Generatione*, Leiden, 1739), and later reprinted in London together with an English translation (*Experiments and Considerations on the Generation of Plants*, London, 1747).

Logan's work was known to the European Botanists of his time and Sachs gave him full credit for his discoveries. However, Sachs states in a foot note that he was unacquainted with Logan's Essay and based his account of Logan's contributions on Koelreuter's citations. This essay has not been readily available. Harshberger[18] reprinted it in part, unfortunately with modernized spelling and punctuation, and Roberts[29] has quoted from it extensively.

Logan actually performed his experiments some years before 1739, for on November 20, 1735, he sent a detailed description of them to Peter Collinson, who submitted the account to the Royal Society. His letter to Collinson was published in the *Philosophical Transactions,* number 440. The following extract is the earliest record I have found of Logan's investigations.

Some Experiments concerning the Impregnation *of the* Seeds *of* Plants, *by* James Logan, *Esq; Communicated in a Letter from him to Mr.* Peter Collinson, *F. R. S.*

Philadelphia, Nov. 20, 1735.

SIR,

As the Notion of a Male Seed, or the *Farina Foecundans* in Vegetables is now very common, I shall not trouble you with any Observations concerning it, but such as may have some Tendency to what I have to mention—And, first, I find from Miller's Dictionary, that M. *Geoffroy*, a Name I think of Repute amongst *Naturalists*, from the Experiments he made on *Mayze*, was of Opinion, that Seeds may grow up to their full Size, and appear perfect to the Eye, without being impregnated by the *Farina*, which possibly, for ought I know may in some Cases be true; for there is no End of Varieties in Nature;—but in the Subject he has mention'd I have Reason to believe it's otherwise, and that he applied not all the Care that was requisite in the Management.

When I first met with the Notion of this Male Seed, it was in the Winter Time, when I could do no more than think of it; but in the Spring I resolved to make some Experiments on the *Mayze*, or *Indian Corn*. In each Corner of my Garden, which is forty Foot in Breadth, and near eighty in Length, I planted a Hill of that Corn, and watching the Plants when they grew up to a proper Height, and were pushing out both the Tassels above, and Ears below; from one of those Hills, I cut off the whole Tassels, on the others I carefully open'd the Ends of the Ears, and from some of them I cut or pinch'd off all the silken Filaments; from others I took about half, from others one fourth and three fourths, *etc.*, with some Variety, noting the Heads, and the Quantity taken from each: Other Heads again I tied up at their Ends, just before the Silk was putting out, with fine Muslin, but the Fuzziest or most Nappy I could find, to prevent the Passage of the *Farina;* but that would obstruct neither Sun, Air, or Rain. I fastened it also so very loosely, as not to give the least Check to Vegetation.

The Consequence of all which was this, that of the five or six Ears on the first Hill, from which I had taken all the Tassels, from whence proceeds the *Farina*, there was only one that had so much as a single Grain in it, and that in about four hundred and eighty Cells, had but about twenty or twenty-one Grains, the Heads, or Ears, as they stood on the Plant, look'd as well to the Eye as any other; they were of their proper Length, the Cores of their full Size, but to the Touch, for want of the Grain, they felt light and yielding. On the Core, when divested of the Leaves that cover it, the Beds of Seed were in their Ranges, with only a dry Skin on each.

In the Ears of the other Hills, from which I had taken all the Silk, and in those that I had cover'd with Muslin, there was not so much as one mature grown Grain, nor other than as I have mentioned in the first: But in all the others, in which I had left Part, and taken Part of the Silk, there was in each the exact Proportion of full Grains, according to the Quantity or Number of the Filaments I had left on them. And for the few Grains I found on one Head in the first Hill, I immediately accounted thus: That Head, or Ear, was very large, and stood prominent from the Plant, pointing with its Silk Westward directly toward the next Hill of *Indian* Corn; and the *Farina*, I know, when very ripe, on shaking the Stalk, will fly off in the finest Dust, somewhat like Smoak. I therefore, with good Reason, judged that a Westerly Wind had wafted some few of these Particles from the other Hill, which had light on the Stiles of this Ear, in a Situation perfectly well fitted to receive them, which none of the other Ears, on the same Hill, had. And indeed I admire that there were not more of the same Ear than I found impregnated in the same manner.

As I was very exact in this Experiment, and curious enough in my Observations, and

James Logan

COLONIAL GOVERNOR AND EXPERIMENTALIST

James Logan was among the first to put to the test of experiments the many conjectures regarding sex in plants which were current in the early Eighteenth Century. By isolating maize and protecting the silks from pollen, Logan showed that "farina" from the maize tassel was necessary to produce viable seed.

this, as I have related it, is truly Fact, I think it may reasonably be allowed, that notwithstanding what M. *Geoffroy* may have deliver'd of his Trails on the same Plant. I am positive, by my Experiment on those Heads, that the Silk was taken quite away, and those that were cover'd with Muslin, none of the Grains will grow up to their Size, when prevented of receiving the *Farina* to impregnate them, but appear, when the Ears of Corn are disclosed, with all the Beds of the Seeds, or Grains, in their Ranges, with only a dry Skin on each, about the same Size as when the little tender Ears appear fill'd with milky Juice before it puts out its Silk. But the few Grains that were grown on the single Ear, were as full and as fair as any I had seen, the Places of all the rest had only dry empty Pellicles, as I have described them; and I much question whether the same does not hold generally in the whole Course of Vegetation, though, agreeable to what I first hinted, it may not be safe to pronounce absolutely upon it, without a great Variety of Experiments on different Subjects. But I believe there are few Plants that will afford so fine an Opportunity of observing on them as the *Mayze*, or our *Indian* Corn; because its Stiles may be taken off or left on the Ear, in any Proportion, and the Grains be afterwards number'd in the Manner I have mentioned.

John Bartram

I am indebted to Professor Rodney H. True for calling my attention to the following letters collected by William Darlington and included in his book, *Memorials of John Bartram and Humphry Marshall, with Notices of their Botanical Contemporaries,* published in Philadelphia in 1849. Darlington, unfortunately, did not always print the letters in full and often omitted just the passages which today we consider the most important. Most of these letters are now in the possession of the Pennsylvania Historical Society and I have been able to supplement Darlington's selections from his original source. I have not, however, been able to find what appears to be the crucial letter written by John Bartram to Peter Collinson dated April 29, 1740. An undated Ms. in Bartram's handwriting probably includes the essential experiments described in this letter. It may even be a fragment of the letter itself, or more probably a rough draft from which the letter was copied. The writers and recipients of the letters were:

1. James Logan, "President of the Council, and Chief Justice of the Province of Pensilvania."
2. John Bartram, the celebrated Philadelphia Botanist; first to establish a Botanical Garden in America for the cultivation of both native and exotic vegetation.
3. Peter Collinson, F. R. S. and F. S. A., London merchant and Quaker, an importer of plants.
4. Colonel William Byrd of Virginia, explorer, author and founder of the City of Richmond.

1736. James Logan to John Bartram
(Darlington, p. 307)

Friend J. Bartram:

Last night, in the twilight, I received the inclosed, and opened it by mistake. Last year PETER sent me some tables, which I never examined till since I last saw thee. They are six very large sheets, in which the author (Linnaeus) digests all the productions of Nature in classes. Two of them he bestows on the inanimate, as Stones, Minerals, Earths; two more on Vegetables, and the other two on Animals. His method in the Vegetables is altogether new, for he takes all his distinctions from the *stamina* and *styles*, the first of which he calls husbands, and the other wives. He ranges them, therefore, under those of 1 husband, 2, 3, 4, 5, 6, 7, 8, 9, 10, 12, 20, and then of many husbands. He further distinguishes by the styles, and has many heads, under which he reduces all known plants.

The performance is very curious, and at this time worth thy notice. I would send it to thee, but being in Latin, it will want some explanation, which, after I have given thee, thou wilt, I believe, be fully able to deal with it thyself, since thou generally knows the plants' names. If thou wilt step to town tomorrow, thou wilt find me there with them, at E. SHIPPEN'S, or J. PEMBERTON'S, from 12 to 3. I want also to

say something further to thee, on microscopical observations.

Thy real friend,

J. LOGAN.

Stenton, 19th of June, 1736.

1737. Peter Collinson to John Bartram
(Excerpt, Darlington, p. 107)

London, December 20, 1737.

Dear Friend:

I shall now consider the remaining part of thine of July 19.

The magic lantern is a contrivance to make sport with ignorant people. There is nothing extraordinary in it, so not worth thy further inquiry.

Thee art still desirous of a magnifier for flowers. Pray make this complaint to J. LOGAN, and try his thoughts. As thy inquiries seem in some measure to be owing to him, and as thee art his pupil (which no man need be ashamed of), no doubt but he will furnish thee with suitable instruments for that purpose, in order to render thy discoveries more perfect—so undoubtedly more to his satisfaction. . . .

The references in the following to Dr. Tschiffely and to Byrd's letter of March 23rd enable us to place Bartram's communication in 1739. The greater part of Bartram's letter has been published (Darlington, 1849). The most important portions, however, were omitted by the modest Darlington. These I have italicised as they contain one of the passages which enable us to identify the genus investigated by Bartram as *Lychnis*.

1739. John Bartram to Col. Wm. Byrd
(Bartram Papers, Vol. I, fol. 19)

Dear Friend Coll Byrd:

I r'cived thy kind letter by ye post last winter; and another dated March ye 23d, which I received by ye hand of thy friend doctor Tschiffely, whom I received very kindly and made as welcome as my present Circumstance would afford for thy sake having no other acquaintance than thine and another recommendation. I have this spring made several microscopical observations upon ye malle and femall parts in vegetables to oblige some ingenious botanists in Leyden, who requested that favour of mee which I hope I have performed to their satisfaction and as a mechanical demonstration of ye certaity of this hypothesis of ye different sex in all plants that hath come under my notice. *I cant find that any vegetable hath power to produce perfect seed able to propagate without ye conjunction of malle seed any more than animals and by a good microscope ye malle and femall organs is as plainly discovered.* I have made several Successfull experiments of joyning several species of ye same genus whereby I have obtained curious mixed Colours in flowers never known before but this requires an accurate observation and judgment to know ye precise time *when ye femall organs is disposed to receive ye masculin seed and likewise when it is by ye masculin organs fully perfected for ejection.* I hope by these practicall observations to open a gate into a very lar(g)e field of experimental knowledge which if judiciously improved may be a considerable addition to ye beauty of ye florists garden.

The next letter from Peter Collinson to John Bartram has also been published by Darlington (1849) and again I have italicised the portion he omitted.

1740. Peter Collinson to John Bartram
(Bartram Papers, Vol. II, fol. 53)

London, July 23d, 1740.

Dear Friend:

I had the pleasure of thine, of April 29th, 1740. Thy experiment of the usefulness of the *Farina*, is very curious and entertaining, where plants of a class are growing near together, they will mix and produce a mingled species. An instance we have in our gardens, raised by the late THOMAS FAIRCHILD, who had a plant from seed, that was compounded of the Carnation and Sweet William. It has the leaves of the first, and its flowers double like the Carnation—the size of a Pink—but in clusters like the Sweet William. It is named a *Mule*—per analogy to the mule produced from the Horse and Ass.

Writing on these matters, brings to mind the Papaw—an Indian fruit—which in our stoves is produced in great plenty. On this tree, is very remarkably distinct, male, female, and hermaphrodite blossoms, which are very extraordinary to see: but whether the

last is an assistant in generation, or is a sport in nature, is not yet agreed . . . .

Dr. WITT'S hollow-leafed Lavender, is, no doubt, the Side-saddle flower; but what relation it has to *Lavender*, I must leave to him. The plant with tricolor leaves, I am well assured, is your fine *Clinopodium*. Our late severe winter has carried all mine off; so pray send me some more seed—and of the *Lychnis* with Crosswort laves . . . . *The Flesh Colour** Lychnis *does not appear but in its Roome One with a pale Blew Flower phaps this may be that from Susquehannah a new one and very sweet sented which I esteeme much—and I want when thee increases it, that with a white flower with a Red Spott in the Center, and that* Lychnis *with a small white flower* . . . .

What curious experiments did Bartram recount in his letter of April 29, 1740, which caused Collinson to comment, "When plants of a class are growing near together, they will mix and produce a mingled species," and to follow the comment by a description of the then twenty-three year old hybridization experiment of Thomas Fairchild? The inference is obvious: i. e., that Bartram described his own work on species crossing, the same work to which he referred in his letter to Colonel William Byrd.

The following is transcribed from an undated Ms. in Bartram's handwriting which is now in the possession of the Pennsylvania Historical Society (Bartram Papers, Vol. I, fol. 20). The Ms., consisting of a frayed sheet of paper with a large fragment torn from a corner, shows signs of being very hastily written, i. e., words inserted between the lines, words duplicated, letters small and less clearly formed than usual. The paper had been torn before Bartram wrote on it, so it seems improbable that he ever intended to include it in a letter. The Ms. has the appearance rather of a preliminary draft, possibly of the missing letter to Collinson.

. . . . have had for 3 year a lychnis which produced flesh colored flowers male female upon several plants, one large plant which produced malle over most part of summer but produced no seed which ingaged my perticular notice and I observed that that seed was produced which ye sent me distinct male & female plants a year after I had sent from eng(land) some of ye seed of white lichnic which produced male & female flowers distinct upon different plants. It happened that one female plant flowered neare two weeks before any male flowered which I obsrved daly and observed that tho there was no stamina or anthera in ye flower yet ye capsula was filed with perfect seed which I sowed as soon as it was ripe which came up very well in a few days but by that time that ye capsula of this first flowering white lychnis was near full grown then several malle plants flowered in ye same bed. I then puled of all ye capsulas that was set before any of these male flowers of ye white lic(hnis) appeared & that which was produced after ye male blosoms opened and shed their farina: for I concluded that either that this female white lychnis must be impregnated with ye male red lychnis which grewde 10 yards of it if so then it must pertake of ye nature & color of ye red one or else we should be pusled to reconcile ye ye hypothesis of nescesity of ye male & female parts as after it had happened according to my expectation for it produced flowers great deal paler then ye red & as much higher coloured then ye white. But ye seed of ye white lychnis that was produced after ye male plant flowered produced plants which bore flowers as white as thair original. Moreover I gave docr witt one plant of ye female lychnic which flowered plentifuly with him & produced ye capisula but containing onely something like ye husks of ye seed but no vegitive life on them, by which it apears that ye male parts of vegitables is realy as nexcesary to vegitation.

Chronological List of Important Contributions to Our Knowledge of Hybridization and Sex in Plants, 1676-1761

The following table lists the work of those botanists who discovered by observation and experiment that plants re-

*Not skin-color. In 1740 "flesh-colour" meant light red, like the "meat" of the watermelon.

produced sexually and that it was possible to cross different varieties and species and secure new forms. It is easier to evaluate the contributions of Cotton Mather, Paul Dudley and John Bartram and the re-dated work of Philip Miller and James Logan by placing them in chronological sequence with the investigations of the other contributors.

1676—Grew in an address before the Royal Society, recorded his own belief and that of Sir Thomas Millington, that the stamens are the male organs of the plant and that the pollen acts as vegetable sperm.

1694—Camerarius in a letter to Valentin describes his experiments which proved that pollen is necessary for seed development in *Mercurialis*, *Morus*, *Rianus* and *Zea Mays*.

1703—Moreland tried to discover how pollen influenced the ovules.

1714—Geoffroy reported that the seeds of *Zea mays* and *Mercurialis* are infertile when no pollen reaches the stigma.

1716—Cotton Mather reported certain observations on *Zea Mays* and recorded (1) wind pollination, (2) hybridization (variety cross) and (3) the resemblance of some of the progeny to the male parent; he also reported a cross (?) between *Cucurbita Pepo* and *C. maxima* (?)

1717—Bradley removed the anthers from Tulips, which consequently produced no seed. He also noted spontaneous hybridization in several varieties of *Pyrus Malus* and *Auricula* (*Primula Auricula*).

1717—Fairchild crossed *Dianthus caryophyllus* with *D. barbatus* and found that the progeny resembled both parents. This was the first recorded artificial hybrid.

1721—Miller described (1) several instances of spontaneous hybridization in *Brassica* and the resulting mixed and variable progeny; (2) insect pollination in *Tulips*; and (3) sexual reproduction in *Spinacio* and *Cucumis*.

1723—In a garden in Stenbrohuld, according to Linnaeus, all male flowers were removed from a plant of *Cucurbita Pepo* which, consequently, bore no fruit.

1724—Dudley described wind pollination and variety crosses in *Zea Mays*.

1735—Linnaeus commented on the artifices resorted to by gardeners to obtain hybrids in *Tulipa* and *Brassica*.

1735—Logan showed by controlled experiments that pollen is necessary for the formation of grain in *Zea Mays*.

1739—Bartram, in a letter to Col. Wm. Byrd, states that he has crossed species (*Lychnis*), obtained strange hybrid forms and hoped to develop new horticultural varieties.

1749—Gleditsch fertilized a female palm tree (*Chamaerops humilis*?) in Berlin with pollen brought from a male tree in Leipzig, whereupon the palm produced fertile seed for the first time.

1750—Haartman records forms which he assumes on taxonomic grounds to be natural hybrids in the genera *Veronica*, *Delphinium*, *Saponaria*, *Actaea*, etc.

1761—Koelreuter systematically investigated hybridization and produced artificial hybrids in *Nicotiana*, *Kedmia*, *Dianthus*, *Matthiola*, *Hyoscyamus*, etc.

Summary

A number of early records of plant hybridization made by English and American botanists have been unknown to the historians of the science, while others have been referred to in a manner which minimized their importance. Certain other observations which helped to prove that plants reproduced sexually have been recorded, but mis-dated.

1. The first record we have of spontaneous hybridization is a letter written by Cotton Mather on September 24, 1716, to James Petiver, F. R. S. The account was printed by Mather in *Religio Philosophica: or The Christian Philosopher*, London, 1721. He described variety crossing in *Zea Mays* and the pollination of squash by gourds.

2. The first recorded artificial hybrid resulted from a cross, *Dianthus caryophyllus* × *D. barbatus*. The investiga-

tor who made this cross should be known as Mr. Thomas Fairchild, of Hoxton, not as "a gardener of London." Fairchild's hybrid is described by Bradley (1717), Miller (1731), Collinson (1740), and Darwin (1781).

3. Philip Miller was the first to describe insect pollination (in tulips). The date of his observation was neither 1751 (Sachs) nor 1731 (Green), but was at least as early as 1721. In that year he wrote to Bradley (Oct. 6) and Blair (Oct. 19 and Nov. 11) and described not only insect pollination in tulips and sexual reproduction in spinach, cucumbers and melons, but also spontaneous hybridization in cabbage when different varieties were grown near one another.

4. Paul Dudley in 1724 described wind pollination and variety crosses in *Zea Mays.*

5. James Logan in a letter to Peter Collinson dated November 20, 1735, described his famous experiments on pollination in *Zea Mays.* He later published the results of his investigations in Leiden (1739) and London (1747).

6. John Bartram stated in a letter written in 1739 to Col. Wm. Byrd that he had crossed species within a genus and obtained a mixed and variable hybrid progeny. An undated manuscript in Bartram's handwriting contains an account of hybridization in *Lychnis.*

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Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the reader of the JOURNAL *will be reviewed in later numbers.*

THE FRENCH RACE: Theories of its Origins and their Social and Political Implications Prior to the Revolution. *By* JACQUES BARZUN, Ph. D., Sometime Mitchell Fellow in History, Columbia University, Perkins Scholar in Political Science, Instructor in History at Columbia University. 275 Pages. 14 Chapters. Price, $4.25. Columbia University Press, New York. 1932.

Theories are all one can have of the origin of most races. The *facts* are too meager and too scattered to get very far. The *theories* which the French hold as to their racial origin influence their actions and thus are as important sociologically as *facts*. It will surprise many "Pros" and "Cons" in the "Nordic Superiority" wrangle to learn that it has been going on at least since the second century A. D.

BEITRAGE ZU EINER GENOTYPISCHEN ANALYSE DER ACKERBOHNE, VICIA FABE L. *By* M. J. SIRKS (Instituit voor Plantenveredeling, Wagening, Holland). With 428 Pages, 43 Illustrations, 100 Tables, Roy. 8vo. Reprint from Genetica Vol. CIII. Price 22 Guilders. XI Chapters. Martinus Nijhoff, The Hague.

All about the genes of the broad bean.

INHERITANCE. *By* PHYLLIS BENTLEY. Pp. 592. Price, $2.50. Macmillan, New York. 1932.

Fictional history of an English family through the Industrial Revolution, the World War, down to these Present Peaceful Times, when the recent mob before Buckingham Palace recalls the mobs that destroyed the first Oldroyd textile machinery in 1812.

INTRODUCTION TO THE HISTORY OF SCIENCE, Volume II, From Rabbi ben Ezra to Roger Bacon. *By* GEORGE SARTON, Associate in the History of Science, Carnegie Institution of Washington. In two Parts. Pp. 1251. Price, $12.00. The Williams and Wilkins Co., Baltimore. 1931.

The conviction that we live in the most interesting and exciting epoch in human history is somewhat shaken by this fascinating, more than encyclopaedic, résumé of science in the Twelfth and Thirteenth Centuries.

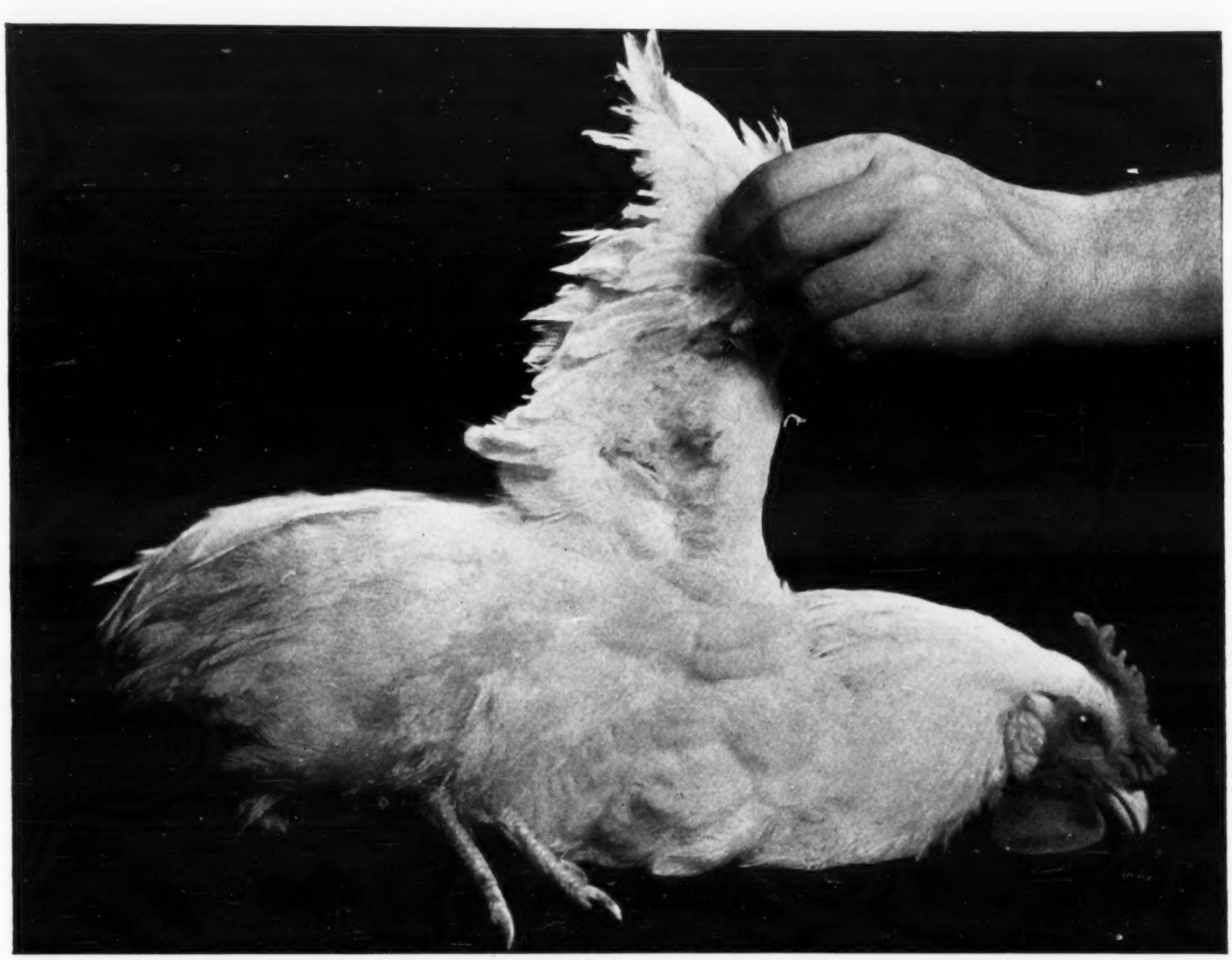
THE MECHANISM OF CREATIVE EVOLUTION. *By* C. C. HURST. Pp. xxi and 365. Frontispiece and 199 Illustrations. Price $6. New York, Macmillan Co., 1932.

If "one picture is worth ten thousand words," this is an encyclopedia of many volumes.

FLIGHTLESS--A HERITABLE VARIATION

In the Domestic Fowl*

D. C. WARREN,
Kansas Agricultural Experiment Station



A "FLIGHTLESS" ROOSTER

Figure 3

A young rooster showing the heritable character "flightless." This causes defects of the large feathers of the wings, which break off, reducing the wing area below that necessary for flight.

THE writer's attention was called to the flightless fowl by Mr. A. O. Robertson of Kansas City, Kansas, who wrote that he had been breeding birds of this type for about 20 years.

Description of Character

The adult showing this character is entirely incapable of flight. Since the wings are useless a flightless bird cannot even mount a roost placed at the usual height and when pedigree hatching from females showing this character the trapnests must be placed near the floor. The inability to fly is due to a defective condition of the flight feathers. The large feathers in other portions of the body are also defective. The defect is in the shaft of the feather causing it to break off when subjected to a slight pressure. The shaft of the portion of the feather remaining is very much roughened showing a tendency for the outer surface to scale off.

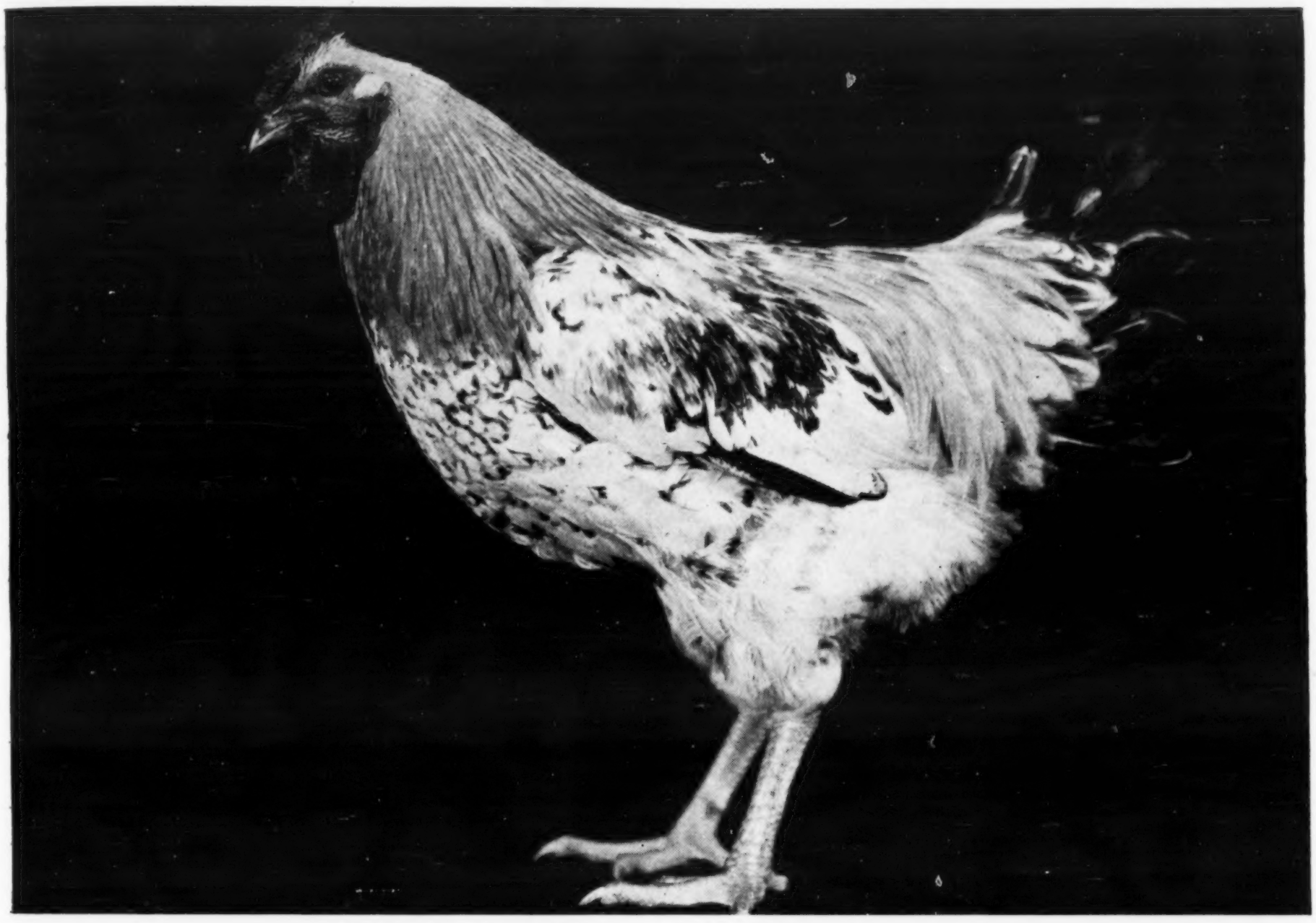
After a molt the new feathers be-

*Contribution No. 68, Department of Poultry Husbandry.

"FLIGHTLESS" DUE TO DEFECTIVE FEATHERS

Figure 4

Above: Young flightless rooster with wings folded, showing "clipped-wing" appearance. Below: "Flightless" feathers and comparable normal wing feathers, showing breakage of the former.



NORMAL ROOSTER

Figure 5

To show wing development in absence of the dominant flightless gene.

gin to break off as soon as they are mature enough to become dry. The flight feathers continue to break until only the base of the shaft and the quill remain in the wing. Except for the condition of the feathers the wing is entirely normal. The large tail feathers also break off but the smaller, more flexible body feathers are not much affected. When the plumage becomes older there is a tendency for a somewhat roughened appearance to develop, probably due to some breaking of the body feathers.

The character may be recognized in chicks as early as one month of age. The spread wing at this time shows the flight feathers to be irregular in length and many individuals will have most of these feathers badly broken. The shafts of the remaining feathers will be very brittle and have the characteristic roughened appearance.

Experimental Results

The original flightless male was mated to a few normal winged females in the fall of 1931 and produced eleven normal and eight flightless offspring. This at once suggested that the character was dominant and that the original male was heterozygous.

In order to check more accurately upon the mode of inheritance of the character the original male was mated to seven Single Comb White Leghorn females and six hybrids from the cross of Black Minorca male by White Leghorn females. The results of this mating are shown in Table I. The totals were 129 flightless to 136 normals. This is a very close approximation of a 1 to 1 ratio which would be expected if flightless were dominant to normal flight feathers and the original male were heterozygous.

The proportion of flightless in each sex is also very nearly equal indicating that the character has good viability.

Five flightless daughters of the original male by normal females were mated to a Silkie Bantam male having normal flight feathers. They produced 15 flightless to 12 normal offspring substantiating the view that the character flightless is a dominant one.

There has recently appeared in this Journal a discussion by Edward McCrady, Jr., of hereditary local alopecia in the guinea fowl. Although the end results are very similar in the two cases, it appears that these characters have a very different physiological basis. In McCrady's guinea, the wing feathers fail to develop while in the chicken they break off due to a defect in the feathers. Alopecia in the guinea is recessive while "flightless" in the chicken behaves as a dominant.

Summary

The foregoing results indicate that the new character, flightless, is a simple autosomal dominant. Its viability is good and it should be a useful character for future studies of the genetic constitution of the domestic fowl.

TABLE 1. A mating of a heterozygous flightless male to normal winged females.

	Daughters		Sons	
	Normal		Normal	
Mother	Feathers	Flightless	Feathers	Flightless
6026A	5	3	8	7
6027A	9	7	5	5
6039A	5	4	4	9
6044A	1	3	1	2
6046A	8	6	12	8
6048A	4	3	6	9
6054A	2	2	1	0
6056A	9	11	15	14
6057A	4	5	10	6
6059A	1	0	0	1
6060A	5	4	5	4
6066A	7	9	7	6
6067A	2	0	0	1
Total	62	57	74	72

129 flightless to 136 normal feathers.

Variation in Carotene Content of Carrot Varieties

Carotene, the raw material from which the liver synthesizes, vitamin *A*, is the substance which gives carrots and other vegetables a characteristic yellow color. McDonald reports in *Science* (July 29, 1932) the result of a carotene analysis of ten varieties of carrots. The yellow varieties of course contain the most, but even the white field carrots contain an appreciable amount. A garden variety, Early Scarlet Horn, with 2.2 grams of carotene per bushel of carrots, was the richest in this substance. The field variety, Isabell's Maude S. contained only 0.027 grams of carotene per bushel—one eighteenth as much. All the samples used in this experiment were grown under greenhouse conditions, as nearly comparable as possible.

A PEACH MUTATION

C. F. KINMAN
Bureau of Plant Industry, U. S. Department of Agriculture

TO THE list of bud variations in deciduous fruit trees that has been brought to the attention of geneticists and horticulturists by the JOURNAL OF HEREDITY,[1, 2, 3, 4, 5] a variation from the normal that was found in a peach tree appears to be a worthy addition.

A few years ago the upper portion of a branch of a Johnson peach tree was observed to have leaves of a yellow color, while those on the remainder of the tree were of the normal green color, typical of the variety. The eight-year old tree on which this branch is growing is in the commercial orchard of F. C. Albright, near Nicolaus, Sutter County, Calif. (Figure 6). The branch with yellow leaves is one of the framework branches and comprises about an eighth of the entire tree. It is three inches in diameter at the point where the mutation occurs, which is six feet from the ground and four feet above the crotch of the tree. Above this point all the leaves produced are of the yellow type, while those below show no traces of this color (Figure 7).

The leaves of the mutant portion of the branch are a lemon yellow color in the spring, and all are apparently of the same intensity of color. When they are a few weeks old, they gradually turn green with those nearest the base of the yellow leaf segment of the branch showing the change in color first and changing most rapidly, the greening becoming gradually less apparent as the top of the branch is approached. By mid-summer, when the leaves are about four months old, this branch might be passed unnoticed by the casual observer if his attention were not directed to the top shoots where the leaves still have a distinctly yellowish cast. By careful examination a distinct difference in color may still be recognized between leaves on the entire mutant portion of this branch and those on normal branches. By middle or late August, when the leaves are about five months old, and throughout the remainder of the season, this difference is not pronounced. Mr. L. B. Scott of the United States Department of Agriculture reports the same phenomenon in the leaves of some first season plants from the mutant branch that he is growing at Shafter, California. The leaves of these became gradually more green as described above, up to late summer, but the lemon yellow color recurred to some extent during the fall.

Measurements were made of both yellow and normal green leaves in 1930, and the latter were found to be a trifle the larger. The average area of yellow leaves was 2.42 square inches and that of the green ones 2.64. Shoots on the yellow branch appear to grow equally as well as those comparably located on any other branch of the tree.

All branches of the tree set heavy crops and bear regularly. Fruit produced by the yellow leaf branch ripens at the same time as that on other branches of the same tree, and measurements indicate that the fruit from the two types of branches is the same size. However, fruit from the mutant branch appears to be a little lighter yellow in color. Stones from fruit on this branch are notably lighter red in color than from other branches and would be classed as dull pink, rather than the usual reddish brown color of normal Johnson peach stones.

The visible leaf peculiarities of the sporting branch are retained by scions taken from it and grafted into other peach trees, as shown by two three-year old grafted trees growing near the parent tree and by one-year old trees on the United States Cotton Field Station grounds near Shafter. In no case is there any apparent influence of the

A MUTANT IN FOLIAGE COLOR

Figure 6

An eight-year-old Johnson peach tree which produces leaves of a lemon yellow color on the upper portion of the framework branch at the left. The arrow indicates the point where the change in leaf color occurs.

Figure 7

Near view of the location where the mutation occurs on the tree shown in Figure 6. The knife indicates the point where the leaf color changes from normal green to yellow.

Figure 8

The union and growth of a yellow-leaf scion from the mutant branch shown in Figure 6 grafted to a normal healthy seedling peach. All leaves of the scion are yellow while all those below the union are of normal green color.

scion on the color of leaves that develop on shoots that grow from the stock portion of these trees. Yellow leafed scions appear to be entirely congenial with the peach in which they are grafted (Figure 8).

Seedlings Yellow-Leaved

The behavior of its progeny adds to the interest of the yellow leaf branch. Thirty-one plants were grown from 100 open pollinated seed that were taken from this branch and planted by Mr. Scott at Shafter. Fifteen of these seedlings had leaves of the same yellow color as the parent branch, while the other 16 have normal leaves. Since these seeds were open pollinated, the

ratios are not significant genetically. The seedlings from the mutant branch and a lot from seed from the normal branches of the tree have grown about equally well. Both yellow and green leafed plants were also obtained from seed from the yellow leaf branch which was planted near the mother tree in Sutter County. All seedlings from the normal branches have green leaves. Blossoms on the yellow leaf branch will be self-pollinated next spring in the hope of securing seed for a new generation.

Aside from the mutant branch and scions taken from it and its seedling progeny, no other tree or portion of a tree with the same leaf color has been found in this orchard, nor have such been found during many surveys of peach nurseries or in orchards in important peach growing sections in California. Since the color difference in leaves of the normal green and the yellow type is as striking in small trees as in larger ones, a large number of trees in the nursery and in young orchards may be satisfactorily surveyed, for the color variaiton is sufficiently striking to be recognized as far as the tree itself may be distinguished. The behavior of material from this branch on propagation indicates that the yellow foliage is not caused by a virus disease or a nutritional condition.

The fact that a portion of the plants grown from seed produced on this mutant branch show the leaf characteristics of the mutant indicates that the reproductive as well as somatic tissues have been affected in this variation, which apparently originated as a bud sport.

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One Anthropologist's Logic

ANTHROPOLOGY AND MODERN LIFE, *by* Franz Boas, Ph.D., Professor of Anthropology at Columbia University; former President of the A. A. A. S. Pages 246. Price, $3.00. W. W. Norton, New York, 1932.

PROFESSOR BOAS remarks that the difference between the biologist and the anthropologist is that the former permits reason alone to reign in his domain, the latter will acknowledge no such complete domination. It does not require even a glance at the title to convince the reader that the book was not written by a biologist. Personally we have no complaint with admitting emotion into our domain, but prefer it seasoned with at least a sprinkling of logic.

We read on p. 173, " - - - the children whose permanent teeth erupt early are also taller on the average than those whose permanent teeth erupt late." Also, "Therefore all the bodily measurements of children of the rich are greater than those of the poor of the same age." With the accuracy of these sweeping statements we are not concerned at present. We are merely accepting them and arranging them in logical sequence. We might say that since it is the taller children who get their teeth early, and since it is the children of the rich who are taller, that as a biologist and a reasoning being, we are led to conclude that it is the children of the rich who get their permanent teeth earlier. Not so the an-

thropologist. With a rare combination of emotion and reason the senses at once (in fact we only turn the page before he senses it), that it is the children of the poor who get their permanent teeth early!

Nor can the biologist, weighted down with reason, scale those educational heights over which the wings of emotion so swiftly carry this anthropologist. We had been under the impression, erroneous apparently, that grading children on the basis of their mental abilities at school was not half bad. If they were too stupid to pass the work they remained behind with children of a younger age until their mental level was reached. But alas! We were mistaken. We are not yet sure just which age is to determine their advancement or their educational rating, but it is to be their *physiological age*. Perhaps they are to be promoted when they get a new tooth, or when they add another inch to their stature, or a pound to their weight. I recall a certain college girl who made a brilliant record, and who did not lose one of her deciduous teeth until she was past 21; also a highly intelligent medical student of 25 who had never developed any hair on his face. Perhaps these students should have been back in high school waiting for teeth and beard to grow. Where would we place those numerous mental deficients whose physiological sexual maturity comes at the normal chronological age? Shall they go to high school because their physiological development entitles them to be there?

So much for education; what are Boas' views on Eugenics? We confess to being an ardent eugenicist; but we also confess to tolerance of the opponent's viewpoint when presented logically; but then Prof. Boas himself admitted that he was an anthropologist, so we are not responsible for any lack of toleration. He does admit that there are certain pathological traits that are inherited, and that "defective classes whose deficiencies are proved by rigid methods to be due to hereditary causes" should be suppressed. But he adds that the "serious demand be made that ---- in each and every case the hereditary character of the trait must be established before it can be assumed to exist." On the other hand, is it not just as imperative that the trait in question be proved beyond doubt to depend upon environment before such dependence can be assumed? If we should not sterilize our mental defective because we have not adequately proved that his defect is hereditary, should we expend millions of dollars upon expensive philanthropic measures designed to alleviate environmental conditions that we have not rigidly proved were responsible for the defective conditions in question?

He objects to eliminating the undesirable strains because of the loss of the normal persons who would have been born along with the undesirable as a consequence of Mendel's second law. If the theory that environment is so largely responsible for all man's ills be true, we have no right to let normal persons be born into families in which others are defective, for at once they are subjected to a very undesirable environment, and thus liable to become defectives themselves. Those who object to the eugenic program talk about the "social rights" of the defectives. They lose sight of the social rights of the normal person. Is he not within his right to refuse support for his neighbor's physically and mentally defective children if it means inadequate support of or actual suppression of his own normal offspring? Has the defective the right to suppress the children of the normal? Yet this is what happens when our modern system of taxation compels the normal man to pay for the food, shelter, even the clothing, of the physically and mentally defective and of the criminal, before he can claim from his own earnings one cent to support his own normal child. There are "social rights" in this matter it is true, but it is time that some advocate of the rights of the normal man should come forward, and demand his fair share of his own earnings to look after his own children.

Only one more statement will be dealt with. To deal adequately with the fallacies in this book would require a volume fully as large, which would be hardly worth while. This statement (it cannot be called argument, because that implies some element of logical reasoning), is as follows. "Another aspect of the problem is of much more vital importance to mankind. The object of eugenics is to raise a better race and do away with increasing suffering by eliminating those who are by heredity destined to suffer and to cause suffering. I believe that the human mind and body are so constituted that the attainment of these ends would lead to the destruction of society. The wish for the elimination of unnecessary suffering is divided by a narrow margin from the wish for the elimination of all suffering." Boas goes on to say that for the development of the race there must be suffering, lest the cessation of all discomfort lead to the degeneration of the race. Would he recommend that we abolish all anesthesia, so that mankind could suffer adequately again for the ennoblement of their souls during surgical operations? If his argument be true why does he insist a few pages earlier upon eliminating all "the social conditions that have raised a poverty and disease stricken proletariat, social conditions which remorselessly push human beings into helpless and hopeless misery?" If hereditary suffering be for the good of the race, that it become not too effeminate, why do away with environmental suffering? Are these poor wretches not contributing to the furtherance of civilization by keeping alive the torch of suffering which sears their bodies but elevates their souls? Why abolish the grinding cruel labor that saps the strength of men and women; why destroy the very agony which is the stepping stone up which man stumbles toward his ideals?

But enough of Prof. Boas. Eugenics he fears may prove a two edged sword; philanthropy has already proved itself one. It is a whole barbed wire entanglement in which mankind is enmeshed, perpetuating the very conditions which it aimed to abolish, through its constant aiding of those who need it to reproduce still more defectives who in turn demand still more philanthropy.

MADGE T. MACKLIN.

University of Western Ontario.

A Friend of the Negro

EMBREE, E. R. Brown America, the Story of a New Race. New Yark, The Viking Press, 1931, pp vi+311.

THE title of Mr. Embree's book is not intended to convey the idea that the American people are destined to become brown in color, or even yellow. Whether or not Mr. Embree believes that the Negroes will eventually be absorbed by the whites is not made entirely clear. Miscegenation, as most students of the subject agree, is less frequent than formerly. But the author remarks that "No races in history have lived side by side over long periods of time without almost completely mixing their bloods; and in the long future, this course may prove to be inevitable in the United States." With this remark the subject is dismissed. Apparently the author is not worried over the outcome, whatever it may be.

The title "Brown America" is chosen to emphasize the fact that the American Negroes are forming a new race, brown in color, and combining the characteristics of whites, Negroes and Indians. The percentage of Negroes having some admixture of white blood is impossible to estimate. It may be reasonably safe to assert that it lies somewhere between 20 and 80 per cent. The white blood of the light colored mulattoes will gradually diffuse through the darker Negro population and eventually produce a brown hue. That the new mixture which is being formed is

(*Continued on page* 478)

NEMATODE RESISTANCE OF PINEAPPLES

Varietal Resistance of Pineapple Roots to the Nematode Heterodera Radicicola (Greef) Muller*

J. L. Collins and H. R. Hagan†
University of Hawaii

TWO varieties of pineapples, Cayenne and Hilo, are grown commercially in Hawaii. The roots of both are subject to attack by the root knot nematode, *Heterodera radicicola*. These frequently become so abundant that they interfere with the normal root development of the plants. To what extent these nematodes damage the plants and how much other, more obscure, conditions contribute to their failure in some areas is not known. The nematode galls are obvious, and the failure of plants has been largely ascribed to their influence. Contrary to this conclusion is the fact that in some areas the roots of pineapple plants were found to be heavily infested with nematode galls, yet the plants remained healthy and vigorous. No economical method has been found to control or prevent nematode attacks on pineapple plants.

The present paper is a report of the results of experiments to test the different available pineapple varieties for resistance to nematode attack.

Review of Literature

Not all of the following papers were available to the writers. Hence it has been necessary to depend on abstracts and statements in other articles for some of the reviews. Such assistance has been properly accredited.

The response of plants to nematode attack

The presence of nematodes on the roots of a plant does not always result in material damage to the host. Indeed nearly forty years ago Vuillemin and Legrain[34] followed a year later by Queva[27] according to Steiner,[31] showed that certain species of plants might even derive distinct advantages in the possession of *Heterodera radicicola* galls on their roots.

The sweet potato is one of the many cultivated hosts of *H. radicicola*. Weimer and Harter,[36] also Poole and Schmidt,[25, 26] agreed that damage, when evident, is more severe on light, sandy soils than it is on other types. Indeed the former writers remarked: "In the plot at Baldwin Park, the Nancy Halls yielded about as well as the Porto Rico; in fact, in this plot, although the plants were fairly heavily infested, the nematodes seemed to have little effect on the yield."

Attacks of *Heterodera schachtii* on potatoes appear to cause some retardation in early growth. Nevertheless, recovery takes place before maturity and ordinarily a normal crop of potatoes is harvested. Miles[18] stated that "From the data given in this table (IV) it is apparent that eelworm intensity has little effect on yield." Others who have arrived at similar conclusions are Morgan,[19, 20] Smith and Miles[30] and Triffit.[33] Miss Triffit's recent work showed that potato plants rather quickly overcame the retardation of early growth. Morgan and Peters[21] do not entirely agree but express their belief that good agricultural practices can counteract, or prevent the damage caused by *H. schachtii*.

Statistical treatment of nematode problems

The only extensive statistical work in relation to plant parasitic nematodes,

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*Technical Paper No. 33 of the Experiment Station of the Association of Hawaiian Pineapple Canners, University of Hawaii.

†Geneticist and Associate Nematologist, collaborators.

found by us, was reported by Smith and Prentice.[29] In their study of numbers of *H. schachtii* cysts in the soil, they adequately discussed their technique and gave the probable errors for their counts. Following their method Smith,[28] also Smith and Miles,[30] determined the cyst populations of soils.

Goodey,[10] too, has presented in tabular form the degree of susceptibility of certain legumes to the attack of *Tylenchus dipsaci,* giving the percentages of healthy and deformed seedlings with the standard errors. However, he concluded that he could have developed a more accurate determination of susceptibility and, in fact, suggested such a method. Steiner[31] (pp. 510-511) offered another comment on the work that also seemed to impair its utility.

Others have given some attention to comparative damage between varieties of plants with tabulation of data but without statistical analysis. Fromme[8] decided that his tabulations designed to reveal relative susceptibility of wheat varieties to infection by *Tylenchus tritici,* showed no significant results. Leukel's[13,14] data gave the comparative damage to several varieties of wheat infested by the same nematode. Kanred alone appeared to be resistant. He concluded also that emmer and spelt exhibited less damage than wheat.

Response of different species or varieties to nematode attack

McClintock[16, 17] working with *Heterodera radicicola,* reported that onions and peanuts were quite resistant. Iron, Brabham, and Victor cow peas are more resistant that other varieties of cow peas. Three varieties of soy beans and certain velvet beans showed less resistance, while cereals and grasses did not appear to be seriously affected. He obtained resistant peach root stock and an immune plum on which he grafted desirable, but susceptible, varieties. However, it was Neal,[22] according to Webber and Orton,[35] who is credited with being the first to attempt to find nematode resistant root stocks. That he clearly recognized the importance of using immune varieties is shown by his statement; "I believe the use of trees that are not susceptible to the root-knot for stocks on which to bud or graft the susceptible varieties is the proper solution of the root-knot problem."

Poole and Schmidt[25, 26] grew about 30 varieties of sweet potatoes in North Carolina fields infested by *Heterodera radicicola.* Weimer and Harter[36] had already completed such an experiment in three localities in California. Both found divergences from normal yield of sweet potatoes in some of the varieties. Unfortunately, for statistical analysis there were no check plots of uninfested soil from which to obtain normal yields for comparison with yields of infested plants. There was also no assurance that the soil planted to these varieties was uniformly inoculated; the percentage difference in yields did not hold true throughout the tests.

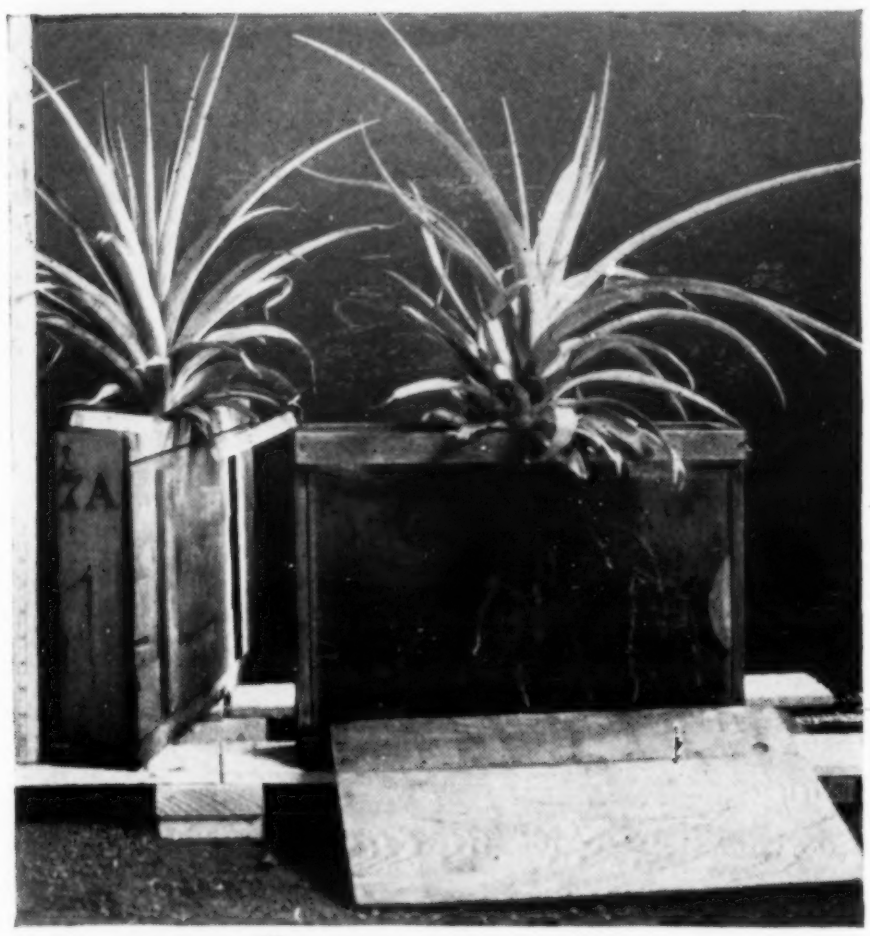
Wilfarth[37] cited cases of apparent immunity occasionally found among sugar beets in fields infested with *H. schachtii* and suggested the selection of such individuals for the propagation of seed. Husfeld[11] later attempted such a program but his 10,000 plants proved to be as susceptible as the population obtained from infested plants.

Nilsson-Ehle[23] tested several varieties of barley for attacks of *H. schachtii.* Chevalier I and II apparently were immune. He considered immunity in these varieties to be conditioned by a single dominant gene.

Numerous writers have commented on the obvious susceptibility of some varieties of plants. Most of them have reported, however, host preference of nematodes rather than differential susceptibility of the host to nematode attack; but the literature has not always carefully distinguished between these two concepts.

Atkinson[1] was one of the first to publish a list of plants attacked by *H. radicicola.* The list was divided into the classes, "badly affected and slightly affected."

Isbell[12] has recently reported the dis-



"ROOT CAGES" USED IN EXPERIMENTS

Figure 9

Root observation boxes used in these experiments. Size 3½×12×16 inches.

covery of a selection in pole snap beans which is immune to the attacks of *H. radicicola*. This appears to be the first instance of immunity or resistance to nematodes noted in *Phaseolus vulgaris*.

Byars, Johnson and Leukel[5] made observations on rye, oats, spelt, and emmer, and remarked that *Tylenchus tritici* severely attacks rye. Byars[4] points out that this nematode parasitizes wheat more extensively than the other cereal grasses.

From this brief review it is evident that numerous observations have been made regarding the influence of nematodes on their hosts. A few writers quite definitely state that there is no evidence that the particular varieties of plants tested are affected to any appreciable extent by nematodes. On the other hand, there is an extensive literature concerning the damage to certain crops by nematodes. Such material has not been considered as it lies outside the scope of this paper. In all the work reviewed, it appears that no attempts were made to determine statistically the relative amount of injury caused by nematodes to different varieties of a single species. Our present efforts are directed toward ascertaining whether the available varieties of pineapples are equally affected.

Experimental Methods

The root observation boxes used in this work were the "root cages" of Comstock[6] that have been modified somewhat in use at this station. The details of their construction have been fully described by Dean.[7] Two are shown in Figure 9.

The plants were grown in a synthetic soil which had been steam sterilized. The planting material consisted of slips, except in the case of the variety known as Lot 520. Two plants were grown in each observation box. Throughout the experiment the plants were grown under the shelter of a lath house.

After the plants had produced roots varying from 5 to 8 inches in length, one half of the plants in each variety was inoculated with nematodes by placing about 50 egg masses on, or just in front of, the growing point of the visible roots. The remaining half was retained uninoculated to serve as a check.

The inoculum used originally came from nematode egg masses and larvae in pineapple field soil. In order to obtain sufficient quantities of egg masses for inoculation purposes, Godfrey's[9] method of nematode propagation was used. Thus, the nematodes passed several generations on Groit and Whippoorwill cowpea roots immediately prior to their transferrence again to pineapple hosts. When inoculation work was started, roots from these plants were washed free from soil. The protruding egg masses then were picked off and placed in beakers of water. Pipettes were used to distribute each lot of eggs over the root tips of the pineapple plants. A heavy and practically simultaneous inoculation of the observation boxes, therefore, was possible.

While no work has been carried out in an effort to determine whether biological strains of *Heterodera radicicola* exist in Hawaiian soils, observations, so far, have not detected any. The fact that all of the varieties of pineapples grown in the experiment showed abundant, and

rather uniform, nematode attack (Table 3, Column 6) is sufficient proof that the inoculum was satisfactory. Nevertheless, some discussion of this subject has appeared in the literature. Tischler's[32] report, as cited by Steiner,[31] and the latter's excellent paper, also, strongly support the conception of biological strains. On the contrary, Bessey[2] and Malloch[15] found no such evidence for this species of nematode. The present paper attempts to throw no further light on this interesting question. It recognizes the possibility of such a condition existing without, however, in any way vitiating the results set forth herein. Finally, it should be noted that only one kind of host was present in each observation box so there was no problem of host selection involved.

The first planting consisted of 30 plants of each of the following varieties: Cayenne, Hilo, Lot 520 (F_1 hybrids of Wild Brazil $\times$ Cayenne), Natal, and Pernambuco. These were started in May 1930. A second group was planted the following September. The varieties then available were Cayenne, Hilo, Ruby, Taboga, and Wild Kailua. The soil was obtained from the same source and other factors were kept, as nearly as possible, the same as in the earlier planting.

As already noted, Cayenne and Hilo are the varieties grown commercially in the Hawaiian Islands. The Lot 520 is a vegetatively propagated hybrid, produced by crossing Cayenne with a wild pineapple from Brazil. It is not grown commercially. The remaining varieties are grown to some extent in other tropical countries.

Six months after date of inoculation, when the plants were approximately 8 months old, the soil was carefully washed from the roots and the following set of records was made.

On check plants:

1. Number of long roots and short roots per plant.
2. Number of old roots and new roots per plant.
3. Length of each root.
4. The amount of fine lateral roots for some of them and notes concerning relative abundance of laterals for all plants.

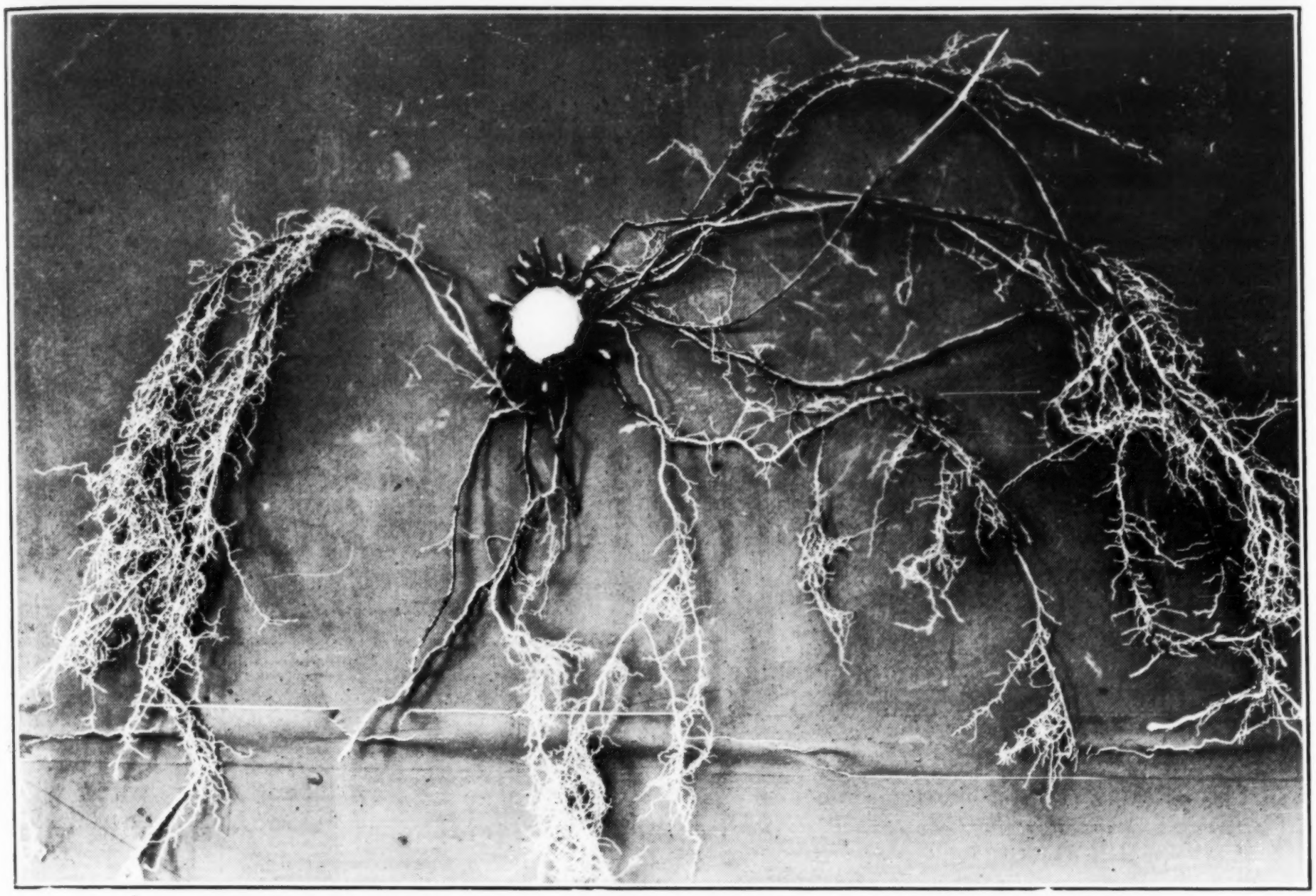
On inoculated plants:

1. Number of long roots and short roots per plant.
2. Number of old roots and new roots per plant.
3. Length of each root.
4. Number of terminal galls per root.
5. Number of non-terminal galls per root.
6. Degree of gall infestation of fine fibrous laterals on each main root.

Due to the fact that it was desired to keep these plants for further observations it was not possible to determine the actual number of small galls on fibrous roots. They were, therefore, estimated on a scale ranging from "trace" to 10. The upper limit, 10, being the maximum infestation considered possible—such a condition not yet being determined. Where less than 5 galls were seen a record of "trace" was made. Five to ten galls were recorded as an infestation of 1, and so on. While this was a rough approximation of the actual infestation, and admittedly one not subject to mathematical analysis, it is felt that it gave a sufficiently accurate picture of the general infestation of lateral roots. These laterals, themselves, varied greatly in abundance in the different varieties so that their numbers, in relation to the small galls upon them, had to be evaluated during the course of our rapid inspection of each root system.

Each plant, after the record of its roots had been made, was placed immediately in the field. After the plant crop is harvested they are to be removed and the roots again examined for degree and severity of infestation.

Throughout this paper the word resistance is employed to designate the tolerance the plant evidences in its reactions to nematode attack. Some plant species are immune to this nematode but no immune pineapple plants are yet known. Some varieties of pineapples, however, are shown herein to be less severely damaged in their roots when attacked. They are considered, therefore, to be more tolerant to nematodes than are others. This tolerance or resistance, when quite pronounced, may



ROOT SYSTEM OF A FIELD-GROWN CAYENNE PLANT

Figure 10

Further growth of visible short roots was abruptly stopped by the formation of terminal galls, produced by nematodes.

serve all practical purposes in growing pineapples where an immune variety cannot be found. Thus, resistance and immunity to attack may mean quite different things but further treatment of this aspect of the subject is unnecessary here. Very satisfactory definitions for these and other terms have been given by Orton.[24]

The authors wish to acknowledge their indebtedness to Eric Ashby, Kenneth R. Kerns, L. H. Kang, and K. Ishiyama who gave valuable assistance in conducting the experiment.

Comparison of Root Systems of Varieties in Check Plants

The ratio of roots to tops and the percentage of fibrous roots was determined for a portion of the check plants of five varieties, data of which are given in Table 1. Root volume was determined both by water displacement and by wet weight; the green tops were weighed. The fibrous roots were separated from the larger roots and the volume determined by the water displacement method. The percentage of fibrous roots for the different varieties was thus obtained.

The largest root to top ratio difference is that between Pernambuco and Hilo, 13.9 ± 2.83. The difference in top to root ratio is approximately 4.90 times the probable error and, therefore, represents a true difference in root systems of these varieties. We should expect differences of the same order as those which distinguish aerial parts of the varieties and Hilo normally produces a larger top growth than Pernambuco.

The percentage of fibrous roots in the different varieties, as shown by the figures in Table 1, is in accord with conclusions drawn from observations of the roots, a condensed summary of which appears later. Lot 520, obviously,

has the largest quantity of fibrous roots and Cayenne the smallest. Natal, Hilo and Pernambuco are about alike.

In comparing the different varieties in regard to number of roots produced, as shown in Table 2, it is evident that differences exist between some varieties and do not exist between others. Thus the difference between Hilo and Taboga is 16.61 times its probable error. Taboga and Ruby have definitely fewer roots per plant than have the other varieties.

The number of roots produced early by a plant may have a bearing upon the future development of its root system in the presence of nematodes. It has been a common observation that in infested fields the early roots ordinarily grow out well and attain considerable length. Later roots, on the contrary, usually are thought to be stopped promptly by severe nematode attack and nearly all of them contain large terminal galls. This quite typical condition is well illustrated in Figure 2. Later in this paper it will be shown exactly to what degree nematodes curtail root growth. At this point it is sufficient to call attention to the fact that considerable variation exists between varieties in the number of roots produced early in the growth of the plants (Table II, Column 5). For example, Cayenne produces 82 per cent of its roots early while Pernambuco sends out 98 per cent of its roots at that time. Hilo, too, may be considered superior to Cayenne in this respect while Natal is inferior, 20 per cent of

TABLE I. Root to top ratio and percentage of fibrous roots in varieties (Checks).

No. of Plants Used	Variety	Wet weight in grams		Ratio	Root Volume cc.	Per cent Fibrous Roots
		Tops	Roots			
2	Cayenne	402	21	19.0 ± --	19.0	26.0
7	Hilo	582	18	32.8 ± 2.69	18.0	35.7
3	Lot 520	715	27	26.7 ± --	25.0	44.0
7	Natal	381	17	22.7 ± 1.22	15.7	30.0
7	Pernambuco	402	23	18.9 ± 0.92	22.0	36.0

TABLE II. Average number, length, and the percentage of old roots per plant and the total root length per plant for check plants.

1.	2.	3	4	5	6
Variety	No. of Plants	Aver. No. Roots	Aver. length	Per cent old roots	Total length of roots per plant
Cayenne	12	39.08 ± 2.48	9.08 ± 0.62	82.00	341.60 inches
Hilo	28	42.57 ± 1.81	10.50 ± 0.21	91.11	435.40 "
Lot 520	14	25.00 ± 1.97	7.50 ± 0.31	83.00	187.50 "
Natal	14	24.80 ± 1.07	12.50 ± 0.02	79.00	310.00 "
Pernambuco	14	34.30 ± 2.26	10.80 ± 0.36	98.00	370.44 "
Ruby	14	16.50 ± 0.81	11.71 ± 0.31	90.90	198.90 "
Taboga	14	11.50 ± 0.50	13.14 ± 0.39	92.54	152.50 "
Wild Kailua	14	20.14 ± 0.70	13.35 ± 0.28	92.55	262.20 "

its roots appearing as new or later produced ones.

Varietal differences in total root length also are pronounced. It is recognized that this feature must be correlated with other factors as well as number of roots and average length of roots. Likewise the size of the root system may have some bearing (other conditions being uniform) on the size of plant and fruit yield. In regard to total length of root system, Hilo exceeds all other varieties tested with 435.5 inches of roots while Cayenne ranks third with a root length of 341.6 inches. It is interesting to note that Taboga and Wild Kailua roots are of approximately equal average length, but while the former has only 11.5 roots per plant with total length of 152.5 inches, the latter produces 20.1 roots and a total length of 262.2 inches.

(To be concluded)

"IAROVIZATION"

A New Factor in Plant Breeding Technique

IT IS announced that the difficulties experienced by plant breeders in crossing plants of different blooming dates can now be overcome by the employment of a series of new methods worked out at the Odessa Institute of Genetics and Plant Breeding by Prof. T. D. Lissenko.

The details of Lissenko's methods have not been published outside of Russia but the strikingly novel ideas upon which these methods are based together with some rather convincing illustrations are presented in the June, 1932, number of *Der Züchter* by A. A. Sapegin, the Director of the Odessa Institute.

According to Lissenko, the development of an organism manifests itself chiefly in well recognizable consecutive stages each of which is characterized by its own specific morphology and physiology. The individual developmental stages require for their completion different complexes of environmental conditions just as different biotypes require such different environments for their normal development.

Lissenko has worked out, for instance, the temperature and temperature duration requirements for the first development stages of wheat. In speaking of the first development stage he, apparently, has in mind the pre-germination stage, i. e., the period when the swollen seed is just going through the most primary life processes of metabolic activity.

Light and darkness are other essential components of the environmental complex, particularly in the case of the so-called "short day" types of plants. Darkness for these "short day" types is just as indispensable as light and is, especially, essential for the completion of the vegetative and the transition into the reproductive stage. But what is particularly striking is Lissenko's alleged discovery that this darkness does not necessarily have to alternate with light and that it can be applied in the required quantity all at once. In other words, a plant can receive its full quota of darkness in its early pre-seedling stage and can then be grown to maturity in uninterrupted light. This is not only true of darkness but also of temperature and other factors making up the environmental complex of the plant. Lissenko's experiments with wheat, millet, corn, cotton, sorgo, sudan grass, vetch, and soybeans have shown that the influence of the direct external factors and their duration, necessary for the completion of the vegetative and for the transition to the reproductive state, is required in exactly the same manner whether the plant is in the state of an ungerminated seed which has been only slightly stimulated into its primary life processes by soaking or whether it is in the state of active vegetative advance.

This occurrence has been designated by Lissenko with the term "Iarovizatzia" which, unfortunately, cannot be anglicized. "Iarovization" has already been adapted by Soviet plant breeders for the purpose of shortening the vegetative period thus accelerating their variety breeding work two or three times. The matching of the blooming periods in parent plants, the breaking up of the persistent vegetative stage in some hybrids and bringing into maturity any exotic plant in a totally strange environment are some of the other possible applications claimed for "iarovization" in plant breeding work.

The direct application of "iarovization" to agricultural practice is also manifold. It has already been employed in Soviet Russia it is claimed in 1930 and 1931 on an area of over a quarter of a million acres in cases where winter wheat has frozen out or where it has been killed by drouth. By "iarovizing" their remaining seed material and sowing it in the spring the Soviet farmers were enabled to obtain a normal yield of high quality grain. In regions where hot, dry winds prevail during the critical period of seed formation the possibilities offered by "iarovization" in shortening the vegetation period are especially important.

Lissenko asserts that "iarovization" does not by any means supplant plant breeding but rather supplements it, that all work on "iarovization" was developed on the basis of differences in the degree of reaction of different biotypes against changes of external conditions. Lissenko expects that in the future "iarovization" will become superfluous through breeding, i. e., that such varieties will be obtained which will require no more "iarovization."

A. J. Bruman.
Division of Foreign Plant Introduction
U. S. Department of Agriculture.

For Livestock Breeders

FERTILITY AND ANIMAL BREEDING, *by* F. H. A. Marshall and John Hammond, 3rd Edition, pp. 1-50. Ministry of Agriculture and Fisheries, Bulletin No. 39. London, 1932.

For the breeder of domestic animals and pet stock this bulletin should be exceptionally useful at it describes in detail the structure and functions of the male and female genital organs as well as the various steps in the process of reproduction.

The authors, although primarily interested in science and research, have a thorough understanding and appreciation of both the scientific and practical phases of animal breeding. Accordingly they have presented their subject from the viewpoint of the practical breeder. The publication is written in an understandable style, technical terms are used sparingly and carefully defined when used, and illustrations are presented where needed.

Breeding management designed to insure normal reproduction is discussed from several different angles, and the discussion is extended to include the selection and mating of breeding animals, the effect of nutrition on reproduction, and the rearing of the offspring. Considerable attention is directed toward the pathology of reproduction, and corrective methods to be employed in case of breeding disorders are outlined. The comparative anatomy, physiology and breeding habits of domestic and pet animals and fowls, together with the practical problems specific to each, are presented in considerable detail.

Many theories and fancies, some of which appear to be well supported by scientific data and others which appear to be questionable or without foundation, are also discussed. In some instances theories are defended by the authors which are subject to question by other investigators, yet the publication is written in an exceedingly interesting and instructive style, is well organized and brief, and should be of great value to the animal breeder.

W. W. Swett.
U. S. Bureau of Dairy Industry.

EFFECT OF STORAGE TEMPERATURE ON DATE OF FLOWERING

In the Paperwhite Narcissus

DAVID GRIFFITHS and R. C. WRIGHT
Office of Horticultural Crops and Diseases, U. S. Department of Agriculture

INVESTIGATIONS during the past three years have been conducted into the effect of storage environment on the development of the flower in the Paperwhite narcissus. This season about two dozen lots of bulbs have been employed in various ways in both commercial storage and controlled temperatures in refrigerated storage. In these investigations with Paperwhites the point at issue is one of effect of the storage temperature on time of flowering and growth of plants. There is none of the element of keeping quality involved, for the Paperwhite bulb will keep in any storage temperature from 36° F. to the limit of the temperature conditions obtaining in the various situations in which they have been handled from South Florida to the District of Columbia. These bulbs do not rot as do the Dutch stocks.

No attempt will be made to go into details now except with reference to five flats of 35 bulbs each of Florida grown stocks. These are emphasized because they point to a method of treatment which may possibly be of commercial importance, and which is of some interest to breeders and to students of effect of environment.

It should be noted that in all cases it was information on the effect of the storage environment on the comparative date and other flowering characteristics that was mainly sought; consequently it was not practicable to attempt to get plants in early. On the contrary, a handling was practiced which was considered to be average and such as was best calculated to meet the requirements of both early and late lots.

Planting (in flats) occurred September 22 to 25 for all but one or two of the lots mentioned here. After planting, the flats were wet down and placed outside covered with straw until the tops began to appear. On October 20 all of them were placed on the benches of the greenhouse and carried without heat until the first one came into blossom, after which a night temperature of 45° to 50° was specified. Bulbs from both North and South Carolina were also carried at various temperatures mostly for the entire storage season, but a few were taken from storage and potted for forcing August 1.

As a general principle, the effect of cold storage is a dwarfing one on the Paperwhite as on other varieties of narcissus. In all lots stored at 36°, 40°, 50°, and 55° F. for the entire season there was an intolerable amount of dwarfing. Those stored at 36° invariably made practically no growth of top and but little of root. The condition from 40° was but little better. From 50° to 55° the dwarfing was still too great to be commercially tolerable. At 60° there was still dwarfing, although without checks for comparison the plants might be considered normal. Not until a temperature of 70° was reached was the growth considered normal of the bulbs submitted to controlled temperatures for the entire storage season, from June to the end of September.

Bulbs were subjected to 55° and 60° respectively from June 12 to August 1, then potted and placed in a cellar where the temperatures ran 70° to 80° during August and slightly lower in September. In both cases

EFFECT OF STORAGE TEMPRATURES

Figure 11

Reaction of Florida grown Paperwhites to various constant temperatures, August 15 to September 20, following normal storage. Upper left, 60°F, blossomed November 13; right, 55°, blossomed November 8; middle left,, 50°, blossomed November 17; right, check, blossomed November 25; front, 36°, blossomed December 8.

the growth was erratic and unsatisfactory, and the flowering and rooting subnormal.

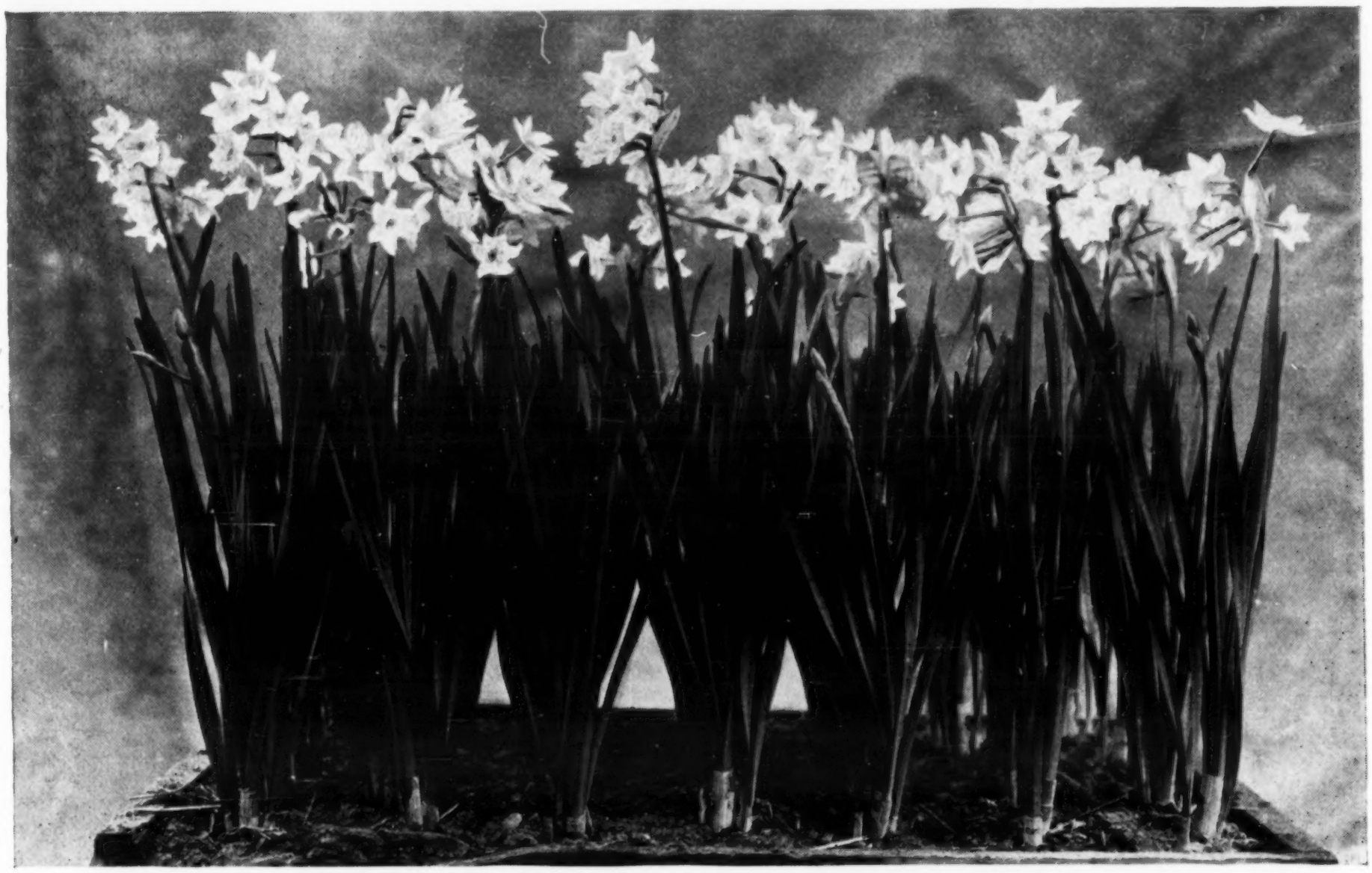
The bulbs of the main Florida stock which are of most importance were received the middle of June and put in ordinary commercial storage on Arlington Experiment Farm, Rosslyn, Va., until August 15, then subjected to various controlled storage temperatures until September 22 to 25, when they were potted. The handling from this date on was identical with that accorded to those in cold storage the entire season.

The behavior of these bulbs was very interesting and important from the commercial point of view. A summary of their behavior follows:

In the reactions there are two points not brought out above that are particularly interesting, namely, the dwarfing of the plants at 50° and 55° was not noticeable in the size of the leaves but in the number of shoots which came out of a bulb. A most unlooked-for reaction was an apparent curtailment of shoot development in the lots stored at 50° and 55°, and a negligible if any diminution at 36°

Table I—Summary of Reactions of Florida Grown Paperwhites to Storage Temperatures, August 15 to September 22

Temperature in Degrees F.	*Date of Flowering*	*Condition of Plants*	*Condition of Flowers*
36	12/8	slightly dwarfed	slightly dwarfed
50	11/17	subnormal	subnormal
55	11/8	subnormal	subnormal
60	11/13	subnormal	subnormal
Check	11/23	normal	normal



EARLY BLOSSOMING OF TREATED BULBS

Figure 12

The flat shown in upper right of previous figure when in height of blossom. The bulbs were exposed to a storage temperature of 55°F. The plants blossomed two weeks before the check lot.

and 60°. The flat stored at 36° was very much retarded, but when it came into full flower about the middle of December the shoots were as numerous as in the checks, but the stems were shorter although not detrimentally so. It will be seen that the reaction is a complicated one.

The defect of low temperatures on the inflorescence was more striking but it ran parallel with the effect on the plants. The greatest floral injury occurred in the bulbs stored at 50° and 55°. It was such as not to be commercially tolerable at 50°, but only slight at 55°. The diminution in the size of the flower, while evident, was not serious in either the 50° or the 55° temperatures. The diminution in the number of florets at 50°, however, was serious. It is thought that there was some injury of this kind at 55°, but it was not serious enough to attract attention when the flat was in blossom. This kind of reaction was wholly unexpected; consequently, since the inflorescence was commercially acceptable, the flowers were cut at full anthesis with satisfactory spikes of blossoms. A comparison of the number of florets from 50° and the check, however, has been made and shows a yield of 284 and 430 respectively, each having 35 bulbs. The flat stored at 36° also had 35 bulbs, but produced 416 florets.

A reading on single pots of six bulbs each, South Carolina grown but stored under controlled temperatures as designated below, throws further light on the subject.

One pot in this lot was stored at 55° and was planted September 1, placed in the rooting cellar to September 22, and then carried like the others. It produced six spikes and 30 florets from six bulbs, starting to open November 22.

It is realized that the practical man who has followed us up to this point may think that cold storaging of Paperwhites produces very compli-

cated results. So it does! But there appear to be a few lessons that can be learned from the data.

1. That storing the bulbs at 55° F. for six weeks before potting the latter part of September induces a precocity of flowering of over two weeks with a floral modification which is commercially tolerable. In other words, the treatment appears to be practicable if the flowers are needed earlier than they come naturally.

2. That a temperature of 36° F. for the same length of time retards flowering beyond the point at which flowering occurs from commercial storage, with floral injury that is commercially negligible.

3. That storage of Paperwhites below 60° F. for the entire storage season from June to the end of September should not be considered.

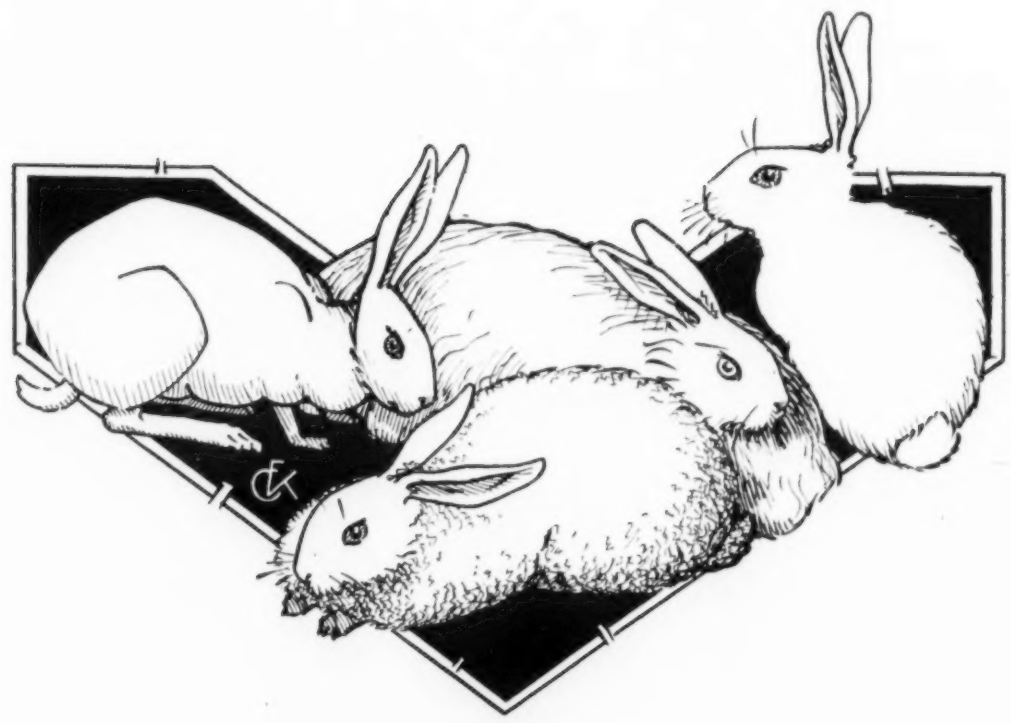
4. All this seems to coincide in a general way (but with modifications) with the work of Blaauw[1] who found that to induce precocity of flowering in the tulip it was best to submit it to a temperature of about 70° for a month and then to 50° until potting.

1. Blaauw, Prof. A. H. Proc. Konin. Akad. van Wetenshcappen te Amsterdam, 10: 10, p. 1342, 1926.

Table II—Effect of Storage Temperatures on South Carolina Grown Bulbs Stored from June 15 to September 22.

Temp. in Degrees F.	*Date of Flowering*	*No. of Spikes*	*No. of Florets*
50	.....	....	no growth
60	11/12	6	32
70	11/28	6	70
Check	11/27	6	87

Editor's Note: The results reported above on the effects of storage temperature on narcissus bulbs evidently parallel the iarovization experiments of Lissenko, in that a temperature treatment of dormant bulbs has drastically affected their later development. Since Mr. Bruman's note called attention to Lissenko's work, American research along this same line has come to our attention. A report of this is anticipated in an early number of the Journal.



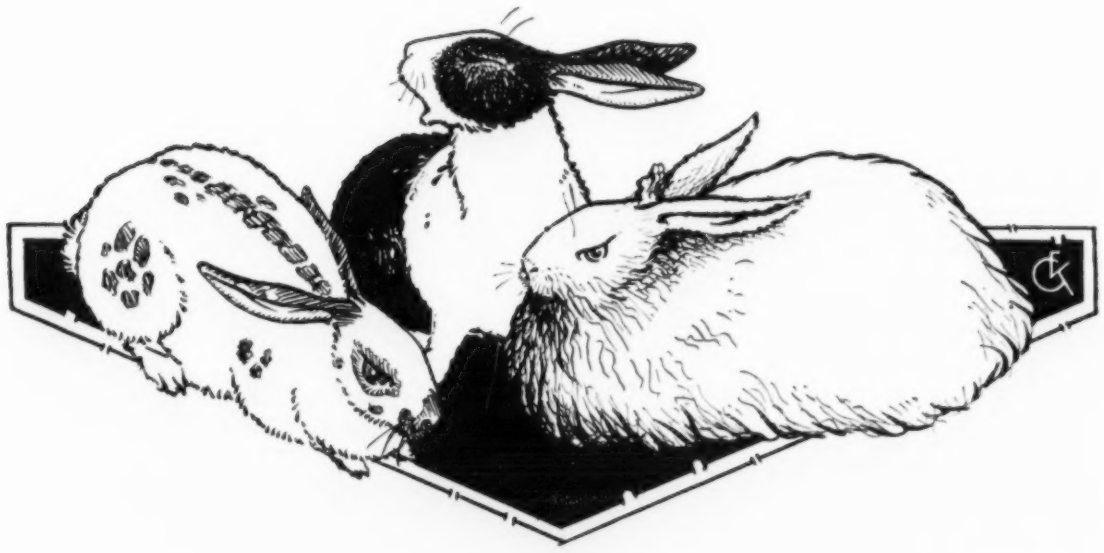
GENETIC ART

ART is based on the physical and living world about us, and on our knowledge, theories, and beliefs of how it is put together. The Madonnas and saints of Raphael and Michaelangelo and their contemporaries reflect the religious outlook of the Middle Ages. It is more than a coincidence that the greatest agricultural and botanical encyclopedias of ancient and medieval times were Chinese and that the art of China is based on the more common species of cultivated and wild plants of that region. To what we may trace the modernistic, broken mirror era of Twentieth Century art will be left to the reader's conjectural ability.

The program of the recent Genetic Congress was adorned with a series of conventional designs based on such important genetic media as the pea, the fruit fly, the mouse and the maize plant. These decorations, which added so greatly to the artistic merit of the program owed their origin to the combined inspirations of Dr. E. M. East and Dr. Clyde E. Keeler. Dr. Keeler has generously consented to prepare a series of genetic sketches and designs to be used when opportunity offers in the make-up of the JOURNAL. The first contributions to this series, reproduced herewith, and on the cover of the JOURNAL, deal with two genera that are Keeler's special pets, and the chosen field of much genetic research—mice and rabbits. It is hoped that in later issues we will be privileged to publish sketches illustrating the genetic peculiarities of other organisms.

The design appearing on the cover of the JOURNAL illustrates the genetic relationships of dominant spotting in mice. A single dose of this gene produces the type of mouse illustrated by the mother—with a white blaze on the forehead, a white belly, and possibly variegation elsewhere on the body. When the character is inherited from both parents so that the genes are present in double dose it produces a lethal form of anemia. The offspring who are so unfortunate as to be blessed with a double inheritance of this gene are so weak that they are not able to compete for their rightful share of their mother's milk. This is, however, not the sole reason for their failure to survive, because even when artificially fed they soon die.

The four rabbits shown on the opposite page illustrate various combinations of characters. On the left is a rex rabbit, next to it an angora, at the right a normal and below an angora-rex. The angora and rex

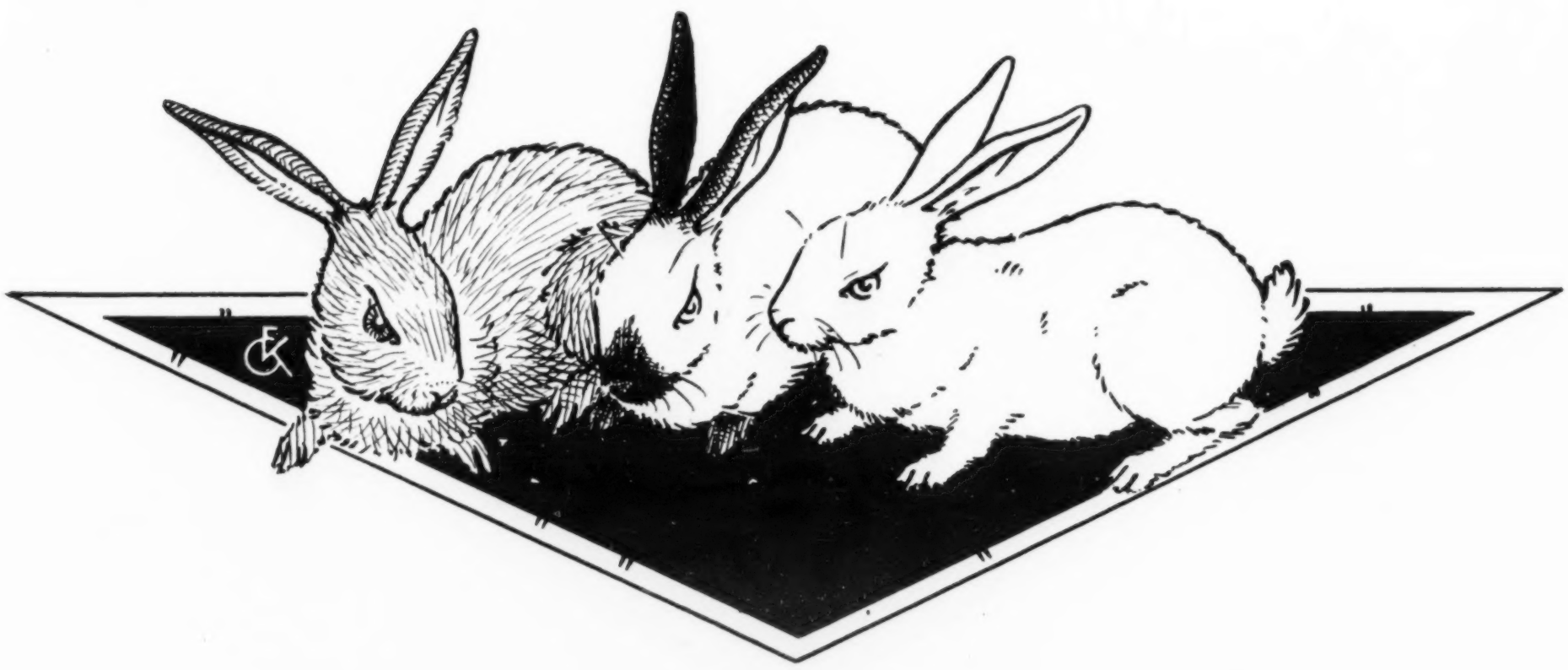


genes are inherited independently. Rex is a recessive to normal hair development, characterized by the absence of the guard hairs and reduction in length of underfur hairs.

On the preceding page are shown three linked genes in the rabbit. These produce two coat patterns, English and Dutch and the angora coat. The characteristic results of three allelomorphic genes are shown below. These are chinchilla, Himalayan, and the albino. The gene for normal coat color, that is, for the coat color of the wild rabbit, is also an allelomorph in this series. A given rabbit can inherit any two genes in this series, but cannot inherit more than this number.

A pair of linked genes in mice is shown above. These are the genes for recessive hairlessness and recessive piebald. A normal mouse is also shown. This form of spotting is different from the dominant spotting in mice discussed in an earlier paragraph.

R. C. C.



TWINNING, SEX RATIOS, AND GENETIC VARIABILITY IN BIRTH WEIGHT IN SHEEP

A. B. CHAPMAN and J. L. LUSH
Iowa State College

THE literature on the sex ratio in sheep shows fairly close agreement in the findings of seven different American, British, French, and German investigators who observed more than 80,000 sheep in all. With the exception of two reports, both based on small numbers, ewe lambs outnumber ram lambs in all cases. The figure most generally given is a little more than 49 males to a little less than 51 females. The ratios reported are based on numbers born, or secondary sex ratios. The primary ratio, or proportion of the sexes at conception, would be practically impossible to obtain and the tertiary sex ratios or proportions among those reaching maturity were not studied. Some additional information about sex ratios, twinning, and genetic aspects of birth weight comes from a recent examination of data from the Iowa Station flock.

The Iowa Agricultural Experiment Station has maintained a small flock of sheep under farm conditions for many years.* The flock is used mainly to study the effects of different rations for wintering the ewes. It was started in 1914 with ewes of mixed breeding showing Leicester, Merino, Lincoln, and Shropshire blood. Purebred Hampshire rams have been used on the flock ever since and, as the replacements for the ewes have been chosen from within the flock, the flock now consists entirely of high grade Hampshires outwardly indistinguishable from purebreds. The rams have always been purchased and usually have been so little related to each other that almost no close breeding has been practiced. There have been a few exceptions to this and even a few matings of sire to daughter. Usually only one ram is used and is kept in service for only one season. Records of 1,019 lambs born from 1915 to 1930, inclusive, were studied.

Sex Ratio and Litter Size

Two hundred and sixteen of the lambs were born as singles, 722 were twins and 81 were triplets; or, of the ewes which lambed, nearly 36 per cent bore singles, nearly 60 per cent bore twins and less than five per cent produced triplets. Among the singles 47.7 per cent were males, among the twins 49.6 per cent were males and among the triplets 40.7 per cent were males. Four hundred and ninety-three of the 1,019 lambs were males which is 48.4 per cent of the total. The differences in the percentages of males found among singles, twins and triplets were not statistically significant although the percentage of males among triplets is low enough to suggest that it might be worth further investigation.

About five per cent of the lambs were stillborn. Almost the same proportion were stillborn among the single and the twin lambs. The triplets included a slightly higher proportion of stillbirths than the singles or twins. Twenty-two of the stillborn lambs were male and 27 were female—an insignificant difference between the two sexes in pre-natal mortality.

Do Identical Twins Occur in Sheep?

Three sex combinations are possible among twins; namely, both rams, ram and ewe, or both ewes. If sex determination is purely a matter of chance determining which kind of spermatozoon

*Under the direction of Dr. John M. Evvard and later of Prof. C. C. Culbertson.

fertilizes the ovum, the distribution of the three kinds of twins should be according to formula $(p+q)^2$ where p is the proportion of rams and q is the proportion of ewes among all lambs born in the flock. One possible cause for deviation from this rule of the binomial square would be the frequent occurrence of identical (monozygotic) twins which would increase the proportion of like-sexed twins. Indeed this is the common basis for estimating the proportion of identical twins among human births.

Table I shows the actual numbers of twins of each sex combination in this flock and the numbers expected on the basis (1) of the sex ratio among these twins, (2) of the sex ratio in this whole flock, and (3) of a general sex ratio of about 49 per cent males reported from many other flocks.

The actual numbers agree with the expected numbers very closely. There is a slight excess of twins of unlike sex instead of a deficiency, as would be expected if identical twins were at all frequent in sheep. This is in agreement with Laplaud and Garnier who report a twin ratio of 17 ♂♂ :37 ♀♂ : 16 ♀♀ from the Vaux-de-Cernay flock. This, of course, proves only that the proportion of identical twins in sheep is low. Such twins perhaps may occur at rare intervals. In a group of data no more extensive than ours something like 20 per cent of the like-sexed twins would need to be identical for the difference between actual and expected numbers to be statistically significant. Hence as the matter rests now, these data give no positive indication of the existence of *any* identical twins in sheep, yet they do not rule out the possibility that such twins may occur, at least occasionally. The probability of their occurrence is not large enough to warrant the investigator's going to much extra expense or trouble in seeking like-sexed twins for pairing against each other in order better to control genetic differences in experiments on nutrition and management of sheep.*

Genetic Influences on Birth Weight

Part of the variation in birth weight was caused by genetic differences between the lambs, part of it by definite environmental influences and part by what, for lack of a better term, may be called accidents of development. The data permit some crude estimates of the relative importance of these causes of variation.

The analysis consisted of comparing the standard deviations of individual birth weights found when all lambs are studied as a single group with the corresponding standard deviations found within classes when the population was divided according to the independent variable being studied. Table II summarizes the calculations for the weights of litter mates as compared with the weights of all twin lambs. The squared standard deviation or variance of birth

TABLE I—Actual and Expected Numbers of Twinnings of Each Sex Combination

Kind of Twin	Observed	Expected		
		from ratio in twins	from ratio in flock	from general ratio
Both rams	87	89	85	87
Ram and ewe	184	180	180	180
Both ewes	90	92	96	94

*Since this was written a paper by Clark has appeared (*Proc. Amer. Soc. An. Prod.* for November, 1931, pp. 207-209) quoting a twin ratio of 38 ♂♂ :67 ♂♀ :34 ♀♀ from the Alexander Graham Bell flock, a ratio of from 87 ♂♂ :187 ♂♀ :83 ♀♀ from a Merino flock reported by "Bernadin," and announcing a ratio of 129 ♂♂ :273 ♂♀ :121 ♀♀ from the flocks of the University of Minnesota. In all save the small Bell flock the twins of unlike sex actually outnumber (although insignificantly) the twins of like sex, thus making it appear still less possible that identical twins occur frequently among sheep.

weights among all the 174 twin males was 2.57, while the variance found within the litter for birth weights of twin males was only 1.33. In short the variance was 48 per cent less when the study was restricted to lambs born in the same litter than it was when all twin males were included. Expressed in more familiar terms, 48 per cent of the squared standard deviation in birth weights disappears when "litter" is "held constant." The corresponding figure for twin ewes is 46 per cent and for twins of unlike sex is 44 per cent. The weighted average is slightly more than 45 per cent.

It is worth while to inquire more precisely into what is included under the term "litter." The effects of differences in litter size do not enter into the problem at all since the analysis is confined to lambs all born in litters of two.

First of all, litter mates have the same sire and dam and should thus be somewhat more like each other genetically than other lambs are. Secondly, litter mates develop in the same uterus contemporaneously and therefore are subjected to influence by the same general variations in the nutrition or other physiological conditions of the dam during their fetal development. Thirdly, litter mates are, of course, born in the same year. All lambs born in the same year are subject to the same influence of weather variations, differences in management and general influences of that kind which might not be the same for lambs born in different years.

What then, could cause litter mates to differ in birth weight? In the first place, there are genetic differences between full sibs. In a random bred population and for characteristics without dominance, the genetic variance between lambs from a single pair of parents should be just half of the genetic variance between all lambs. While these lambs were not strictly random bred, yet only a few were very closely bred and only the dams of the first few lamb crops were distinct outcrosses. Hence these data come close to fulfilling the condition of coming from a population bred at random (within the limits of the Hampshire breed). All lambs born in a single year were by the same sire (except for the 1918 lamb crop) and therefore half brothers, yet many different rams were used in the period of these experiments and there are many more differences between lambs having neither parent in common than there are between those having a sire in common. The existence of these half sibs would have a slight tendency to cause more than half of the truly genetic variance to appear in the variance found between litter mates. If dominance were general among factors affecting birth weight the correlation between full sibs would be somewhat lowered. This would tend to cause more than half of the truly genetic variance to be found in the variance between litter mates. Whether dominance is frequent among the factors affecting birth weight is debatable.

TABLE II—Analysis of Variance in Birth Weight Showing the Effect of "Litter"

Group of lambs studied	Variance due to	Degrees of freedom	Sum of squares	Mean square	Portion of variance excluded from remainder
Twin males	"Litter"	86	329.1	3.83	$\frac{2.57-1.33}{2.57}=.48$
	Remainder	87	115.3	1.33	
	Total	173	444.4	2.57	
Twin females	"Litter"	89	348.4	3.91	$\frac{2.67-1.44}{2.67}=.46$
	Remainder	90	129.7	1.44	
	Total	179	478.1	2.67	
Twins of unlike sex	"Litter"	183	828.9	4.53	$\frac{3.15-1.77}{3.15}=.44$
	Remainder	184	325.6	1.77	
	Total	367	1154.5	3.15	

The second cause of differences between the birth weights of litter mates is what we have called accidents of development such as perhaps might arise from one embryo becoming implanted in the uterus in a position more favorable for nourishment than the other.

A third cause of differences between litter mates may be competition between them of such a nature that the one which first gets a slight advantage over the other is thereby enabled to gain a still greater advantage. That competition exists is certain from the fact that twins are not as large as singles and triplets are not as large as twins. That the competition could sometimes result in an ever-increasing advantage to the one which first gets ahead seems plausible, although how a critical experiment could be planned to measure the extent of such competition is not clear.

In short, we find among these twins that slightly more than 45 per cent of the variance in the birth weights of the individual lambs arises from the combined effects of controlled environment, i. e., controlled to the extreme degree of development within the same uterus and of genetic differences which exist between lambs not full brothers or sisters to each other but which do not exist between litter mates. Probably this is a little less than half of the total genetic differences. On the other hand 55 per cent of the variance in birth weights of twins in this flock arises from uncontrollable environment (including the possible effects of intra-uterine competition) and from genetic differences which exist between full brothers and sisters.

If we let h^2 represent the portion of the total variance in birth weight which is truly genetic, e^2 the portion which would disappear if it were possible for all the lambs to be developed in the same uterus contemporaneously without being genetically closely related to each other, and d^2 represent the portion of the variance caused by "accidents of development" (including intra-uterine competition which may not affect all lambs to the same extent), we have the following equations:

$$e^2 + \left\{\frac{h^2}{2} - x\right\} = .45$$

$$d^2 + \left\{\frac{h^2}{2} + x\right\} = .55$$

Two things prevent solution for e^2, d^2 and h^2. In the first place there are only two equations. In the second place we are not sure just how much more than half of h^2 is included in the second equation, i. e., the value of x is unknown although it must be rather small.

A very rough approximation to the value of h^2 can be made by comparing the variance between paternal half brothers and sisters with the gross variance. The basis for that analysis is shown in Table III.

One ram was used each year (except in 1917-18, when two were used). Hence separation of the lambs by year born separates them roughly into groups of paternal half brothers and sisters. The difference in weight of males and females was so slight (although statistically significant) and the proportion of the sexes was so nearly

TABLE III—Analysis of Variance in Birth Weights of Twin Lambs by "Year" and Ration

Variance due to	Degrees of freedom	Sum of squares	Mean square	Portion of variance excluded from remainder
Total	721	2082.8	2.889	$\frac{2.889-2.588}{2.889}=.104$
"year"	15	256.0	17.065	
Remainder	706	1826.8	2.588	$\frac{2.889-2.506}{2.889}=.133$
Ration within year	51	185.6	3.640	
Final remainder	655	1641.2	2.506	

equal in different years and lots that sex differences in birth weight could be disregarded in this analysis. Unfortunately (for our purposes) division by year also separates them into groups exposed to the same seasonal influences or general differences in the kind of rations fed. Thus one cannot be certain how much of the 10.4 per cent of the variance, which disappears when they are thus separated, is due to their having the same sire and how much is due to their having been exposed to the same weather variations or to variations in the general value of all the rations fed in a single season. When the weights are separated according to the rations fed to the ewes within each year, 2.9 per cent more of the variance disappears. If we make the assumption (which seems extreme and cannot be rigorously tested) that the differences between seasons have just as much effect on birth weight as the differences between rations within years, there is left 7.5 per cent of the gross variance to be attributed to the influence of the sire.

This should be about one-fourth of the total genetic fraction of the variance (h^2). Thus h^2 would be estimated at about .30. We regard this as probably an extreme upper limit, since it may include some increase in the general average size of the ewes in the flock and also since seasonal influences may have been more important than ration differences within the year. The yearly average birth weights for twin lambs were as follows:

1915—8.59
1916—8.28
1917—7.47
1918—8.45
1919—9.15
1920—9.38
1921—9.80
1922—9.67
1923—8.74
1924—9.72
1925—9.61
1926—9.96
1927—8.91
1928—9.21
1929—9.05
1930—9.57

These show a downward trend for the first two years, then an upward trend for four years and after that there is no distinct trend. On account of this lack of regular trend, we attribute very little of the variance in birth weights from year to year to any steady change in the ewe flock but the possibility that the yearly variations were caused by seasonal conditions to a greater extent than we have estimated still remains.

Our estimate therefore is that not more than 25 to 30 per cent of the variance in birth weight actually found in these data was genuinely genetic (h^2), while about 30 to 35 per cent was caused by tangible environment (e^2) and something like 40 to 45 per cent arose from intangible environment or "accidents of development" (d^2). These estimates involve so much approximation that they must be very tentative but they seem to be the most accurate we can make from the data in hand. Perhaps some one with access to records from a large flock of a single breed in which several rams have been used each year can make a sounder estimate for that flock. These percentages, of course, would be expected to vary somewhat in other flocks. Thus in a flock where several breeds were kept or in a flock recently crossbred one would expect the percentage of genetic differences to be larger and in a pure line or (which is the nearest thing to that in animal breeding) a highly inbred flock consisting of but one inbred line one would expect the genetic portion of the variance in birth weights to approach zero.

Summary

The sex ratio in sheep is normally about 49 per cent males to 51 per cent females.

Identical twins, if they occur at all in sheep, must be rare.

The genetic portion of the variance in birth weight in the Iowa Agricultural Experiment Station flock of high grade Hampshires can hardly exceed 25 to 30

per cent. The portion of the variance arising from differences in tangible environment is at least 30 to 35 per cent, while 40 to 45 per cent or more arises from intangible environment or accidents of development.

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A FRIEND OF THE NEGRO

(*Continued from page* 458)

"astonishingly uniform in its physical character" is clearly an overstatement. The remark is based upon the conclusions of Herskovits, which the author has taken rather too uncritically. The Negroes imported from various parts of Africa were a very diversified lot, like the ancestors of our white population. There is no adequate evidence that anything very remarkable is happening beyond what might be expected from the fusion of two or three diversified racial stocks.

The greater part of Mr. Embree's volume is concerned with the history and achievements of the American Negro, and the relation of Negroes to whites in different regions and periods. Having grown up in Kentuck with his Abolitionist grandfather, to whom he pays a debt of filial gratitude in one of his chapters, Mr. Embree has long been familiar with Negroes and their ways. In his later position of leadership in the administration of the Julius Rosenwald Foundation, which is devoted mainly to promoting the welfare of Negroes, he has had unusual opportunity of becoming acquainted with the broader problems with which the Negroes are confronted. His training and present position may account in a measure for his adoption of the rôle of apologist for the brown man. He writes with a strong sympathy for the Negro in his struggles against unfavorable conditions, and he writes also with an unusual insight into Negro psychology. "Brown America" deserves to be widely read. As the author explains, there is an amazing lack of knowledge of Negroes, even among people who have spent their lives among them. Mr. Embree's book will be very helpful in leading to a real understanding of our brown neighbors.

S. J. Holmes.

University of California.